UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, •71 September 24, 1910

THE JUDGMENT OF DIFFERENCE

WITH SPECIAL REFERENCE TO THE DOCTRINE OF THE THRESHOLD, IN THE CASE OF LIFTED WEIGHTS

BY

WARNER BROWN

BERKELEY
THE UNIVERSITY PRESS

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 1, pp. 1-71 September 24, 1910

THE JUDGMENT OF DIFFERENCE

WITH SPECIAL REFERENCE TO THE DOCTRINE OF THE THRESHOLD, IN THE CASE OF LIFTED WEIGHTS.

BY

WARNER BROWN.

CONTENTS.

No "threshold" or just noticeable difference can be found. Weber's Law, therefore, should be so stated as to require that the same relative difference, whether large or small, yield the same proportion of right and wrong answers. A comparison of the sensitivity of individuals, or of the perceptibility of differences at various intensities of stimulation or under various time and space conditions, ought to rest upon the complete array of judgments for differences of all amounts from a very large one to a very small one. Inferences for any single stage of difference, whether the "just noticeable" or any other, are liable to correction, for larger or smaller differences may establish other relations.

It is of prime importance that the number and proportional amount of the differences in the series be the same for all the standards, and the method of "just noticeable differences" must meet this requirement.

The law is found to hold only approximately in lifting weights of 50, 100 and 150 grams.

Ordinary practice with correction does not appear to increase the observer's sensitivity to differences in weight, but there is a tendency to reduce the number of the coarser errors, and after work under more exacting conditions it is found that these errors cease almost entirely. Tendencies to fluctuate on the part of the constant error can not be attributed to practice.

The series which yield the greatest number of right answers are found to be most constant, and the differences which are largest are also the most constant. For purposes of comparison, small or "threshold," values should be replaced by large differences with the most favorable constant errors in order to get as low a probable error as possible. Combined series are also to be prefered.

TABLES AND FIGURES.

I.

INTRODUCTION.

It seems reasonable to assume that two stimuli may be so nearly alike that to the unaided human senses they appear identical. A coin that is slightly worn seems the same in weight and thickness as one more worn, although the micrometer or the analytical balance discover a difference. So we speak of indistinguishable stimuli and of differences which are just noticeable, with the obvious implication that there are differences so small that they always pass unnoticed. Nor is this assumption confined to the rough estimates of practical affairs; it finds expression as well in the psychological laboratory, where it is formulated in the method of least noticeable differences, which, in all its forms, seeks to measure the amount of difference which has the character implied in its title. Constantly employed in many fruitful investigations this method yields a characteristic value, that of the threshold. The method and its results are alike dominating features in the development of experimental psychology, but the suspicion is of long standing that the method, by presupposing such a result, is responsible for its appearance.

The notion of a just perceptible difference is clear as long as one considers single judgments in each of which a difference is or is not perceived. The experimenter, however, soon learns that such judgments do not tell the whole story—that a still smaller difference might have been perceived at the time, and that the same difference at another time might be imperceptible. When contradictions between the single judgments arise they can be obviated only by repeated trials; hence the appearance of the

method of constant differences with its various modes of treating the statistical results of the repeated trials.[1]

A measure of the reasonable expectation that any particular amount of difference will or will not be perceived can only be obtained in a direct attack by the constant method upon a complete series of differences, such as that covered in the method of least noticeable differences. This involves carrying out both the standard methods together to a completeness far greater than is usually attempted in either. The result is a survey giving the frequency of each type of judgment for each amount of difference, from a very small one to a large one. In contrast with the calculations or inferences involved in the incomplete forms of the standard methods, this combined form may be called *the direct method.* It affords an obvious way of approaching the relations expressed in Weber's law, and further, of examining the nature of the difference threshold itself.

That the carrying out of a complete exploration, covering by small steps all the stages of difference between the stimuli, is entirely practicable within the limits of time prescribed by ordinary laboratory conditions, will be seen by considering the results of the experiments with lifted weights summarized in the accompanying tables. These results, covering 75,100 single comparisons, were accumulated with an expenditure of about 250 hours in actual experimentation, a time which will not appear extravagant to anyone who is in the habit of counting up the hours put upon a special study. Of this only about thirty-five hours were required for the 14,000 comparisons with minimal differences (Table III, p. 24) which cover what is, in many respects, the essential part of the work. This speed can be maintained with satisfactory results, after only a little practice. Some of this time was wasted because the accumulation of data is greater than the proof of the points at issue requires. Perhaps,

---

[1] On the relation between the methods of just noticeable differences and right and wrong cases, cf. Kraepelin, *Phil. Studien* 6, 1891, p. 493; Müller, *Gesichtspunkte u. Tatsachen d. psychophysischen Methodik*, p. 179. In some cases the difference between the methods can be reduced to a difference in handling identical data; cf. Urban, *The Application of Statistical Methods to the Problems of Psychophysics*, 1908; Holt, *Psych. Rev.* 11, 1904, p. 349.

however, there is no harm in an excess of proof when the ground is new. At any rate, the experimental results accumulate with such amazing rapidity that economy of time offers no excuse for the constant substitution of methods of calculation (often of doubtful worth) in the place of straightforward experimentation.[2]

[2] It is remarkable that among all the elaborate researches in psychophysics there are none which present a full series of differences. The nearest approximations are: Lorenz, *Phil. Studien* 2, 1885, sound intensities; differences by steps of about 8% above and below standard. Merkel, *Phil. St.* 4, 1887, sound; steps not less than 6%. Higier, *Phil. St.* 7, 1892, retinal space; 4 standards; differences of 1, 2, 3, 4 and 5%. Kämpfe, *Phil. St.* 8, 1893, sound; 4 standards; differences by steps of ½% from 0 to 6%. Mosch, *Phil. St.* 14, 1898, sound; only 6 differences including zero. Wreschner, *Meth. Beiträge z. psychophysischen Messungen 1898, Schriften d. G'schft f. Psychol. Forschung,* Heft I, weights; 15 standards; differences by steps of 5%. Keller, *Psychol. Studien* 3, 1907, sound; differences by steps of 6%. Of these only Higier gives more than 100 cases for any one difference. Peirce and Jastrow, *Mem. Nat. Acad. Sci.* 3, 1884, pressure, give series which are longer but marred by strong practice effects.

II.

DETAILS OF THE EXPERIMENTS.

The present experiments are of the simplest sort. There are no obvious peculiarities of method or material to invalidate the general trend of the conclusions, or to prevent the extension of the argument to the judgment of differences between stimuli applied to other senses. Weights are particularly convenient because they are so easily controlled and can be handled with greater rapidity and accuracy than most stimuli.

All the work was done by one observer between October 11, 1908, and August 13, 1909. The observer was without previous experience in this kind of experiment and without prejudice or any definite understanding of the purpose of the experiment or of the experimenter's expectations regarding the outcome.[3]

The weights, consisting of cylindrical tin boxes 2.5 cm. high and 4.5 cm. in diameter, with a ridge around the barrel half-way up, loaded with shot and paraffine, carefully centered and painted black, were lifted between the right thumb and middle finger, from above, with a full-arm movement. They were lifted from a silent, flat pad on the knees of the observer, who always occupied the same comfortable low chair, so that the relative position of hand and weight was kept as constant as possible. The weights were indistinguishable by sight (being labeled on the bottom), but the observer did not look at them nor at the record sheet.

The weights were weighed to .001 gram and checked up frequently, never being found to have changed as much as .01 gram. In the series with minimal differences they were kept true to within .002 gram.

The first set of experiments was performed between October 11, 1908, and March 11, 1909. It comprises the first 22 series of

[3] To the writer's wife, Jessie Milliken Brown, is due the credit for the persevering help as observer which made these experiments possible.

Table I, pp. 14-15, and is broken up into the Groups A to V of Table II, pp. 16-17. These data are used in the discussion of the distribution of judgments, the threshold, Weber's law, time and space factors, practice, and variability. They comprise in all 41,800 judgments.

In this set the differences between the weights ran by steps of one per cent. from 1% to 18% increase over the standard. Accordingly all the judgments were judgments of "greater."[4] A difference of one-half of one per cent, was also included in the series, making nineteen members in all. The space order remained the same throughout an entire group of experiments. Thus the left-hand weight was lifted second throughout Groups A to F of Table II. Three standard weights were used in rotation to avoid heating from the hand. In this set the experimenter knew the position of the weights before they were set down for the observer to lift. It does not appear that this possible source of error, which was necessitated by the procedure adopted, had any bad influence on the results.

At one sitting five series, of 38 comparisons each, were taken, and in each series the order of the weights was determined by chance save that the standard came first as often as it came second. In this way 190 comparisons, taking about forty minutes, were made at each sitting. One or two sittings were had daily. They were seldom more than twenty-four or less than eight hours apart. "Warming up" series were taken only when there was some change in standard or in space order; otherwise every judgment was recorded.

The height and speed of lifting, which were left to the

[4] In many respects it would be preferable to use differences below as well as above the standard. But this involves judging in two sets of terms—"greater" and "less"—*e.g.*, a weight of 97 is either "less" than 100 or 100 is "greater" than 97, but judgments about standard 97 do not really contribute to a series which is started in terms of standard 100. There is as yet no empirical justification for the assumption that one of these judgments is psychologically equivalent to the other; cf. Martin and Müller, *Analyse d. Unterschiedsempfindlichkeit,* pp. 7, 24, etc., and consider how seriously their conclusions would be affected by a personal tendency on the observer's part to give more "less" answers than "greater." Cf. Wreschner, *op. cit.* p. 61. For the present purposes one definite direction of difference and a clear type of judgment based upon that direction is essential, and sufficient.

observer to choose, were fairly constant at about 5 cm. height and a time of about ¾ sec. for each lift, with ¼ sec. between lifts. There was an interval of about 3¼ sec. between pairs and six minutes between series. Judgment—expressed by pushing forward, without further lifting, the weight chosen as heavier—was often indicated simultaneously with the return of the second weight to the pad; it was never delayed more than ½ second. The observer was required to indicate in every instance which of the two weights seemed heavier, and in case no clear difference appeared, to guess. In all not over a dozen comparisons had to be repeated on account of hesitation or inability to decide.

The observer's attention was kept off from the time and space order and at the same time centered upon the main business in hand by avoiding all reference to "first" or "last;" "right" or "left." The one idea "this is heavier" was thus kept to the fore and the observer had only to point out the weight upon which the judgment fell. It will be shown later that this Fechnerian procedure is inferior in some respects to that of Müller and Schumann in which judgment falls always upon the second weight.[5] The procedure adopted is however, the simplest and gives wholly satisfactory results.

The second set of experiments, performed between April 20 and June 7, 1909, consists of the ten series of Table III, p. 24, embracing in all 14,000 cases. The differences ran by steps of 0.2 gram from 97.8 to 100.4. The number of standard weights was now increased to equal the number of pairs (14) in the series, and the pairs were re-mated at frequent intervals. This policy was pursued throughout the remaining experiments (except series 23, Table I) and from this time on the experimenter did not know the relative position of the weights till after the judgment had been passed. The time interval between pairs was reduced to 2 seconds and no regular pauses were made during the sitting.

[5] See p. 50, below.
The advantages claimed by Martin and Müller, p. 185, in the matter of ease in handling the data are wholly imaginary; they fail to bring out the greater uncertainty of the judgment in the Fechnerian procedure as indicated in their tables (p. 187) by a greater number of "equal" answers.

From 250 to 300 judgments were now made at a sitting. These data affect the discussion of the threshold, practice, variability, and other points.

In the third set, performed between June 19 and July 14, 1909, there were twenty pairs of weights, as indicated in Table IV, p. 31. There were 6,000 judgments in the set. The conditions were the same as in the one before save that the observer had to say whether the second weight was *the same or heavier* than the first. These data are used in criticizing the use of equivocal answers and in summing up on the "threshold."

The fourth set is composed of three groups of 200 cases each with nineteen pairs of weights in the set, making 11,400 cases in all (Table VII, p. 42). This work was done between July 16 and August 8, 1909. The data show the effect upon the distribution, of different forms of expressing the judgment, and together with the preceding sets, the effect of the number and magnitude of the weights comprising the experimental series.

The fifth set repeats the conditions of the first set for the purpose of providing a check. It comprises only one series (the 23d in Table I) of 1900 cases and was carried out between August 9 and 13, 1909.

III.

THE DIFFERENCE THRESHOLD.

Just how much significance there is in the concept of the difference threshold will only be known after the accumulation of experimental observations covering a number of complete explorations of the various fields in which judgments of difference are met. The accompanying tables present the results of such an exploration in but a narrow portion of one field.

The results of twenty-two successive experimental series of 100 cases each are given in Table I. It exhibits the number of correct judgments given upon the comparison of each standard weight with other weights differing from it by the amounts indicated, the figures being arranged in series of 50 to exhibit each possible time and space order separately. These data are then combined in the various groups of Table II, some of which are again represented graphically in fig. 1 (page 20).

The principal evidence concerning the difference threshold is in Groups P to V (Table II and fig. 1). Here the standard of 100 grams was compared with each of the nineteen heavier weights 1400 times (Group V), that is, 350 times in each of the four possible time and space orders.[6] Besides the constant and unavoidable illusions due to the order of presenting the weights (these will be considered in detail later), there are irregularities due either to chance or to idiosyncrasies of the observer, as for instance the exceptional number of correct judgments on the difference of 10 or 11 per cent. as compared with 12 or 13 per cent. None of those irregularities, however, obscures in the least

[6] These orders as indicated in the tables are:
Second weight greater than and to the left of the first (Group P).
Second weight less than and to the left of the first (Group Q).
Second weight greater than and to the right of the first (Group S).
Second weight less than and to the right of the first (Group T).
The standard is the lesser weight throughout this set of experiments.

the principal fact that even very slight differences of weight are at times recognized and that each slight increase in the amount of difference brings with it an increase in the frequency with which the difference is recognized. There is no difference down to 1% which cannot be recognized occasionally, and there is no difference up to 18% which will not be overlooked occasionally.[7]

The experimental results can be stated most simply in terms of attention. Not only are there states of critical attention in which very small differences are correctly noted, such small differences that they are usually overlooked, but these same small differences make themselves felt when attention is normal or subnormal. A difference of 1% at any stage increases the number of right judgments. A difference so large as 17% is not perceived as many times as 18% and it must be that it escapes notice when a further increase of so little as one per cent. would turn the balance of judgment.

A difference of 10% between weights of 100 grams and 110 grams yields an average of 93.6 correct judgments per hundred with a mean variation of 3 for the fourteen series of a hundred cases. (Group V, Table II). In other words, the attention was so poor that this difference was overlooked six or seven times in every hundred. Some part of those errors must of course be assigned to irregularities in the physiological mechanism. Such irregularities would result in the misrepresentation of one of the individual stimuli, and in the end, would produce a misjudgment of the relation between the stimuli. But whether the lapses be due to lack of a veracious sensory impression from one or the other weight or whether they be due to a more general inattention and failure to coördinate properly the separate impressions, makes little difference in the end. The result is a failure, through the fault of sensory or central processes, in the

[7] The whole treatment of the method of right and wrong cases by Spearman, *Brit. Journ. Psychol.* 2, 1908, p. 227, rests upon the assumption that no errors will occur if the difference is large enough. There is no indication in the present results that such a difference can be looked for far short of the point at infinity where it is required by the theory of probabilities to lie.

TABLE I.

Number of correct judgments out of 50 for each amount of difference.

Series	Standard in Grams	2nd Weight in Relation to 1st	Amount of Difference in per cent. of Standard 0.5	1	2	3	4	5	6
1a	100	greater, left	27	31	28	36	41	41	43
2a	100	greater, left	20	29	29	35	36	40	43
3a	100	greater, left	25	31	34	33	41	40	37
4a	100	greater, left	26	25	27	34	33	36	44
5a	100	greater, left	26	31	32	40	37	39	46
1b	100	less, left	28	25	34	34	33	33	40
2b	100	less, left	27	27	36	41	38	40	41
3b	100	less, left	30	30	27	27	32	37	41
4b	100	less, left	31	34	27	30	39	40	38
5b	100	less, left	22	22	25	26	35	34	35
6a	50	greater, left	27	33	32	33	40	42	39
7a	50	greater, left	23	24	27	27	35	32	32
8a	50	greater, left	24	25	26	36	34	33	40
9a	50	greater, left	23	28	31	33	38	29	37
10a	50	greater, left	29	22	23	29	40	37	37
6b	50	less, left	22	30	24	29	37	32	37
7b	50	less, left	31	30	33	29	37	36	43
8b	50	less, left	20	26	34	36	38	37	41
9b	50	less, left	30	26	32	33	29	36	41
10b	50	less, left	27	26	32	28	27	37	40
11a	50	greater, right	20	22	27	28	30	35	34
11b	50	less, right	26	25	27	37	40	38	42
12a	100	greater, right	24	26	26	32	40	44	39
13a	100	greater, right	21	28	33	37	31	39	37
14a	100	greater, right	21	22	22	34	42	44	33
15a	100	greater, right	14	25	29	37	29	34	37
16a	100	greater, right	25	27	29	34	39	44	42
12b	100	less, right	38	34	36	35	38	47	46
13b	100	less, right	24	27	37	41	38	47	43
14b	100	less, right	25	27	33	35	36	42	42
15b	100	less, right	29	31	29	39	34	37	42
16b	100	less, right	21	25	31	35	33	39	36
17a	100	greater, left	27	24	29	33	42	37	36
17b	100	less, left	33	27	34	35	35	37	44
18a	100	greater, right	28	30	32	37	32	41	41
18b	100	less, right	20	30	32	33	32	40	43
19a	100	greater, left	36	29	37	36	39	43	43
19b	100	less, left	30	32	32	36	40	45	47
20a	100	greater, right	34	31	33	39	39	37	41
20b	100	less, right	29	30	30	31	36	37	38
21a	150	greater, left	22	27	29	43	36	44	40
21b	150	less, left	25	31	32	39	44	42	46
22a	150	greater, right	25	30	34	36	40	44	43
22b	150	less, right	24	23	29	35	34	39	39
23a	100	greater, left	25	25	29	31	42	42	46
23b	100	less. left	26	33	35	38	34	43	41

7314

TABLE I—*(Continued).*

Number of correct judgments out of 50 for each amount of difference.

Series	Amount of Difference in per cent. of Standard 7	8	9	10	11	12	13	14	15	16	17	18
1a	49	48	46	46	49	47	50	50	49	50	50	50
2a	40	43	44	46	49	47	48	49	48	48	49	50
3a	38	45	47	49	50	48	50	50	49	49	50	50
4a	42	48	48	48	48	50	49	50	49	50	50	50
5a	44	46	45	49	49	50	49	50	50	49	50	50
1b	40	36	42	40	47	39	43	46	47	48	48	48
2b	41	43	47	45	46	47	46	44	50	48	50	50
3b	39	43	36	45	44	45	47	48	46	48	48	46
4b	37	39	42	48	46	45	46	46	49	46	50	48
5b	37	34	43	40	44	42	42	44	47	46	46	50
6a	45	43	44	44	47	49	50	49	49	49	47	50
7a	39	41	32	42	45	43	45	43	48	50	48	48
8a	42	44	44	45	46	49	50	49	48	48	47	50
9a	40	44	46	47	46	46	47	48	50	50	50	50
10a	42	45	42	45	47	46	47	49	49	47	50	50
6b	40	44	36	43	47	41	38	45	44	48	48	46
7b	41	44	45	43	44	48	45	48	46	49	48	50
8b	41	41	47	43	44	43	48	49	48	50	49	48
9b	45	44	37	44	45	46	49	48	49	49	48	50
10b	43	47	47	49	48	49	46	48	49	48	50	50
11a	35	37	44	40	44	42	48	45	48	49	49	47
11b	47	44	44	42	44	49	49	49	49	50	50	50
12a	45	42	47	48	48	49	49	49	50	50	50	50
13a	46	42	48	47	50	49	50	50	50	50	50	50
14a	46	42	46	45	50	48	48	50	49	50	50	50
15a	43	47	44	47	48	49	50	49	50	49	50	50
16a	42	45	44	49	49	50	47	50	50	50	50	50
12b	45	44	45	49	50	49	48	50	50	50	49	50
13b	47	45	46	47	48	50	49	49	50	50	50	49
14b	38	45	45	44	49	48	50	50	49	50	49	50
15b	43	42	45	50	49	48	50	49	49	50	50	50
16b	41	44	41	46	45	48	47	49	48	50	50	50
17a	41	48	44	48	50	50	46	49	49	50	50	50
17b	43	45	45	48	47	47	47	49	49	49	49	49
18a	43	45	47	49	50	48	49	50	49	49	50	50
18b	44	40	47	43	47	48	47	48	49	50	49	50
19a	48	45	49	50	50	49	50	50	50	50	50	50
19b	45	48	43	49	49	49	50	49	50	50	50	50
20a	42	50	49	47	49	50	50	50	50	50	50	50
20b	43	42	39	49	46	46	50	49	49	49	48	50
21a	49	45	49	48	48	49	50	49	50	50	50	50
21b	42	50	46	49	47	49	49	49	50	49	50	50
22a	47	48	48	48	47	49	50	50	50	50	50	50
22b	43	44	43	47	45	45	48	47	49	47	49	49
23a	47	49	47	49	50	50	50	50	50	50	50	50
23b	43	43	46	47	49	46	50	50	48	50	50	50

TABLE II.

Average number of correct judgments for each amount of difference.

Group	Average of Series	Standard in Grams	2nd Weight in Relation to 1st	Differences in per cent. of Standard 0.5	1	2	3	4	5	6
A	1a-5a	100	greater, left	24.8	29.4	30	35.6	37.6	39.2	42.6
	m.v. of the 5 series of 50			*1.9*	*1.9*	*2.4*	*1.9*	*2.7*	*1.4*	*2.3*
B	6a-10a	50	greater, left	25.2	26.4	27.8	31.6	37.4	34.6	37
	m.v. of the 5 series of 50			*2.2*	*3.3*	*3*	*2.9*	*2.3*	*3.9*	*2*
C	1b-5b	100	less, left	27.6	27.6	29.8	31.6	35.4	36.8	39
	m.v. of the 5 series of 50			*2.5*	*3.5*	*4.1*	*4.7*	*2.5*	*2.6*	*2*
D	6b-10b	50	less, left	26	27.6	31	31	33.6	35.6	40.4
	m.v. of the 5 series of 50			*4*	*1.9*	*2.8*	*2.8*	*4.5*	*1.4*	*1.5*
E	1-5 (a + b)	100	left	52.4	57	59.8	67.2	73	76	81.6
	m.v. of the 5 series of 100			*3.9*	*2.4*	*3.4*	*4.6*	*.8*	*2*	*1.7*
F	6-10 (a + b)	50	left	51.2	54	58.8	62.6	71	70	77.4
	m.v. of the 5 series of 100			*4*	*3.6*	*2.6*	*4.7*	*3.2*	*3*	*1.7*
G	9 (a + b)	50	left	53	54	63	66	67	65	78
H	5 (a + b)	100	left	48	53	57	66	72	73	81
I	21 (a + b)	150	left	47	58	61	82	80	86	86
J	11 (a + b)	50	right	46	47	54	65	70	73	76
K	16 (a + b)	100	right	46	52	50	69	72	83	78
L	22 (a + b)	150	right	49	53	63	71	74	83	82
M	9, 11 (a + b)	50	all orders	49.5	50.5	59.5	65.5	68.5	69	77
N	5, 16 (a + b)	100	all orders	47	52.5	53.5	67.5	72	78	79.5
O	21, 22 (a + b)	150	all orders	48	55.5	62	76.5	77	84.5	84
P	1-5, 17, 19 (a)	100	greater, left	26.7	28.6	30.9	35.3	38.4	39.4	41.7
	m.v. of the 7 series of 50			*2.7*	*2.6*	*3*	*1.8*	*2.7*	*1.8*	*3*
Q	1-5, 17, 19 (b)	100	less, left	28.7	28.1	30.7	32.7	36	38	40.9
	m.v. of the 7 series of 50			*2.6*	*3.3*	*3.7*	*4.3*	*2.7*	*3.1*	*2.7*
R	1-5, 17, 19 (a + b)	100	left	55.4	56.7	61.6	68	74.4	77.4	82.6
	m.v of the 7 series of 100			*3.8*	*3.1*	*3.4*	*4*	*2*	*2.3*	*2.7*
S	12-16, 18, 20 (a)	100	greater, right	23.9	27	29.1	35.7	36	40.4	38.6
	m.v. of the 7 series of 50			*4.3*	*2.2*	*3*	*2*	*4.6*	*3.4*	*2.3*
T	12-16, 18, 20 (b)	100	less, right	26.6	29.1	32.4	35.6	35.3	41.3	41.4
	m.v. of the 7 series of 50			*4.5*	*2.4*	*2.3*	*2.5*	*2*	*2*	*2.3*
U	12-16, 18, 20 (a + b)	100	right	50.6	56.1	61.7	71.1	71.3	81.7	80
	m.v of the 7 series of 100			*6.8*	*3.6*	*3.3*	*3.5*	*5.1*	*5.5*	*2.4*
V	1-5, 12-20 (a + b)	100	all orders	53	56.4	61.6	69.5	72.8	79.5	81.3
	m.v. of the 14 series of 100			*6.8*	*3.3*	*3.6*	*3.5*	*3.5*	*5.3*	*2.2*

44 39 31 24

TABLE II—*(Continued).*

Average number of correct judgments for each amount of difference.

Group	Differences in per cent. of Standard 7	8	9	10	11	12	13	14	15	16	17	18
A	42.6	46	46	47.6	49	48.4	49.2	49.8	49	49.2	49.8	50
m.v.	*3.1*	*2*	*1.2*	*1.3*	*.4*	*1.3*	*.6*	*.3*	*.4*	*.6*	*.3*	
B	41.6	43.4	41.6	44.6	46.2	46.6	47.8	47.6	48.8	48.8	48.4	49.6
m.v.	*1.7*	*1.1*	*3.8*	*1.3*	*.6*	*1.9*	*1.8*	*1.8*	*.6*	*1*	*1.3*	*.6*
C	38.8	39	42	43.6	45.4	43.6	44.8	46	47.8	47.2	48.4	48.4
m.v.	*1.4*	*3.2*	*2.4*	*2.9*	*1.1*	*2.5*	*1.8*	*.8*	*1.4*	*1*	*1.3*	*1.3*
D	42	44	42.4	44.4	45.8	45.4	45.2	47.6	47.2	48.8	48.6	48.8
m.v.	*1.6*	*1.2*	*4.7*	*1.8*	*1.4*	*2.7*	*3*	*1*	*1.8*	*.6*	*.7*	*1.4*
E	81.4	85	88	91.2	94.4	92	94	95.8	96.8	96.4	98.2	98.4
m.v.	*3*	*2.4*	*2*	*3*	*.9*	*2.4*	*1.6*	*1*	*1*	*.9*	*1*	*1.3*
F	83.6	87.4	84	89	92	92	93	95.2	96	97.6	97	98.4
m.v.	*1.7*	*2.1*	*4.8*	*2.8*	*2*	*1.2*	*3.2*	*2.2*	*2*	*1.3*	*1.6*	*1.3*
G	85	88	83	91	91	92	96	96	99	99	98	100
H	81	80	88	89	93	92	91	94	97	95	96	100
I	91	95	95	97	95	98	99	98	100	99	100	100
J	82	81	88	82	88	91	97	94	97	99	99	97
K	83	89	85	95	94	98	94	99	98	100	100	100
L	90	92	91	95	92	94	98	97	99	97	99	99
M	83.5	84.5	85.5	86.5	89.5	91.5	96.5	95	98	99	98.5	98.5
N	82	84.5	86.5	92	93.5	95	92.5	96.5	97.5	97.5	98	100
O	90.5	93.5	93	96	93.5	96	98.5	97.5	99.5	98	99.5	99.5
P	43.1	46.1	46.1	48	49.3	48.7	48.9	49.7	49.1	49.4	49.9	50
m.v.	*3.3*	*1.6*	*1.6*	*1.1*	*.6*	*1.2*	*1*	*.4*	*.5*	*.7*	*.2*	
Q	40.3	41.1	42.6	45	46.1	44.9	45.9	46.9	48.3	47.9	48.7	48.7
m.v.	*2.3*	*4.1*	*2.1*	*2.9*	*1.3*	*2.5*	*1.9*	*1.6*	*1.4*	*1*	*1.2*	*1.2*
R	83.4	87.2	88.7	93	95.4	93.6	94.8	96.6	97.4	97.3	98.6	98.7
m.v.	*4.5*	*3.5*	*2*	*3.7*	*1.4*	*2.8*	*2.3*	*1.5*	*1.2*	*1.5*	*1.1*	*1.2*
S	43.8	44.7	46.4	47.4	49.1	49	49	49.7	49.7	49.7	50	50
m.v.	*1.5*	*2.7*	*1.5*	*1*	*.7*	*.6*	*.9*	*.4*	*.4*	*.4*		
T	43	43.1	44	46.9	47.7	48.1	48.7	49.1	49.1	49.9	49.3	49.9
m.v.	*2*	*1.6*	*2.3*	*2.1*	*1.5*	*.8*	*1.2*	*.5*	*.5*	*.2*	*.6*	*.2*
U	86.8	87.8	90.4	94.3	96.8	97.1	97.7	98.8	98.8	99.6	99.3	99.9
m.v.	*2.7*	*1.8*	*2.7*	*2.2*	*1.7*	*1*	*1.8*	*.5*	*.7*	*.5*	*.6*	*.3*
V	85.1	87.5	89.5	93.6	96.1	95.3	96.2	97.7	98.1	98.4	98.9	99.3
m.v.	*3.9*	*2.7*	*2.4*	*3*	*1.6*	*2.4*	*2.5*	*1.4*	*1*	*1.4*	*.8*	*.9*

apprehension of an actual difference between the stimuli which, at a moment of better local or general attention, did not fail to make itself felt.

At 18% of difference such lapses of attention occurred only seven times in a thousand trials, while with only 1% of difference between the weights they occurred 436 times in a thousand. When there are only 7 mistakes in a thousand, as is the case with a difference of 18%, they are readily accounted for as lapses of attention. But there are 11 mistakes at 17% and 16 at 16%; at 7% there are 149, and at 1% there are 436. Are we to say that the explanation which holds for 7 mistakes in a thousand will not hold for 11? There is no very apparent difference between the cases. But why stop at 11 or 16? The case is no different for 149 or for 436. It is unusual, indeed, to find such a multitude of lapses and perhaps another name would better fit the case, but certainly there is no point in the series at which a line can be drawn on one side of which all the mistakes are of one sort while on the other side they are of another sort.

When such a line is taken arbitrarily for certain practical purposes, it may be called, properly enough, the threshold. But in that case the threshold means a difference which can win the attention so many times in every hundred trials. The number of times per hundred which is to be selected as the threshold value is determined by extraneous considerations such as the mathematical advantage afforded by the probable error, or the experimental advantages of such percentages as prove to be least (or most) affected by practice or by order of presentation. The particular number of correct judgments which some difference between stimuli yields is not itself significant; a ratio of 50 or 75 per hundred has no advantage over any other. The various numbers yielded by various differences form a continuously graded series, in which the quality of the judgment must be regarded as a constant, independent of the number of times it occurs.

Even those who define the threshold in unexceptionable mathematical terms as the point where half or three-quarters of

all the answers are right, are however, prone to slip back into expressions which imply that judgments below the threshold, if they do occur, as they must according to the theory of probabilities, are somehow different from those above. They are apt to attribute to the threshold a real existence as a turning-point for judgment—the point where judgments of difference emerge from the region where no difference is felt— and this inclination gives to the mathematical definition a psychological interpretation which does not belong to it.

That Weber's law is so commonly stated in terms of thresholds rather than of 40 or 90 per cent. of correct judgments is only one indication of this silent assumption that the threshold is a particularly noteworthy or critical point in the judgment series. The constant employment of the method of just noticeable differences, as well as the retention of "equal" or "no difference" judgments in the method of right and wrong cases, despite the warnings many years ago of Cattell and Jastrow, has fostered the idea that it is desirable to find a point where a difference in sensation first appears, just as it is deemed desirable to find a point in the scale of physical intensities where a sensation quality first appears. The actual distribution of errors does not, however, indicate that there is a virtue in any particular number of judgments by which one is warranted in asserting that if there were fewer of them they must be different in kind. So far as the threshold implies that 'sub-liminal' differences provoke a judgment different in quality from those due to larger differences, it is a misleading concept, and unsupported by fact.

Even if it be true that a greater feeling of confidence accompanies judgments which are based on considerable differences and that loss of assurance follows upon an increase in the number of errors, it is only a change in the observer's *thought about his judgment* and does not affect the actual frequency of the judgments. The question is, can a difference be perceived or not? It matters little what the confident feelings of the observer may be, so long as he can perceive the true relations some part of the

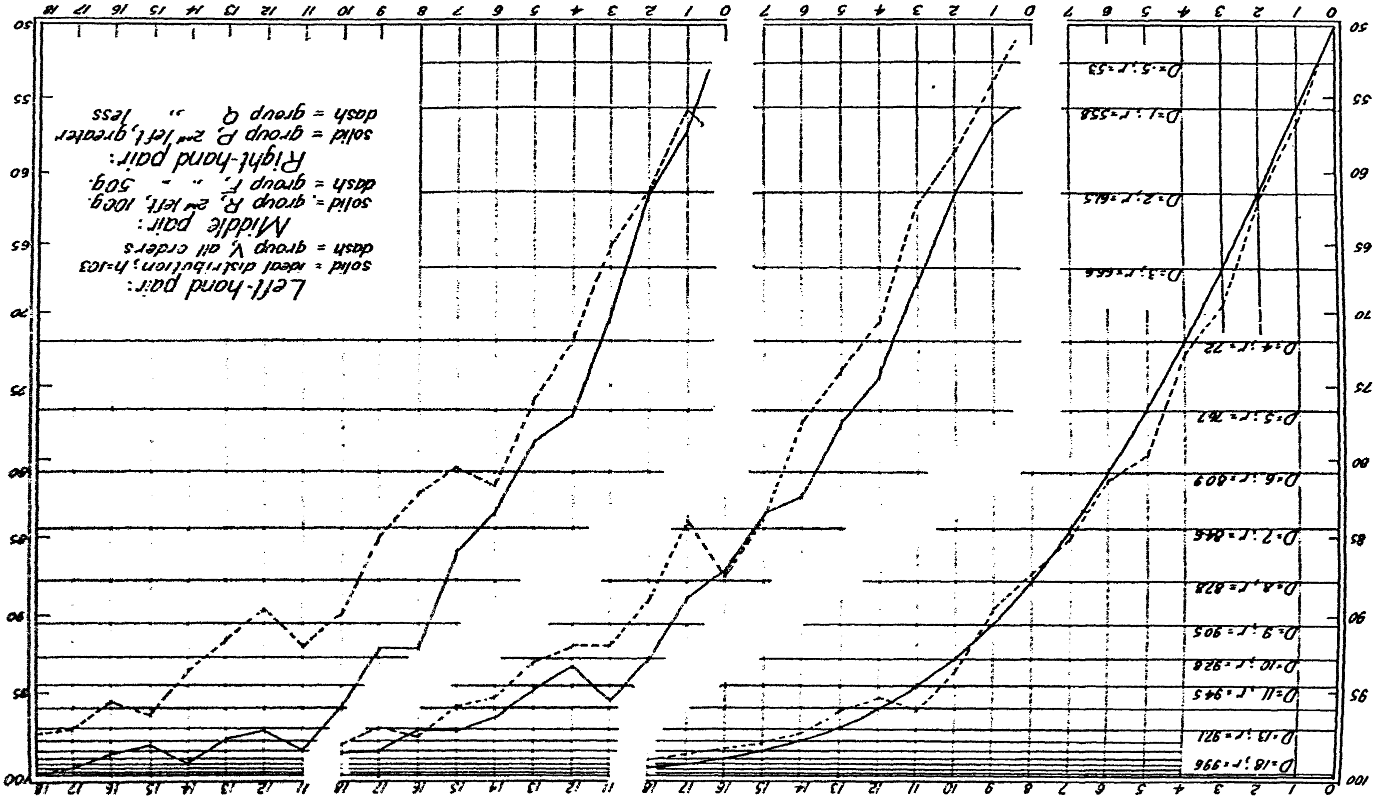


Fig. 1.—The number of correct answers per hundred (measured from 50 up) for each amount of difference from the standard (reading toward the right from either of the three origins at 0). The horizontal lines represent the number of right judgments (r) for each amount of difference (D) for an ideal distribution by chance. Data

time correctly.[8] The experiment shows that even very small differences are perceived correctly part of the time and that they always tend to increase the number of right judgments. There is no evidence of the existence of a difference threshold in the ordinary sense of the term. Its reported appearance is generally attributable to an error of experimental method which will be considered directly; very often a feeling of assurance is mistaken for its appearance; in the remaining cases it will bear restatement in terms of the ratio between the number of right and wrong judgments.

---

[8] Apparently the only attempt to discover the actual course of judgments for very small differences is the classic work of Peirce and Jastrow, *Mem. Nat. Acad. Sci.* 3, 1884. They make out clearly that small differences give a majority of right answers, but the force of this evidence in criticism of the ordinary conception of the threshold has since been almost wholly ignored. They used a method of double comparison and they encountered an improvement with practice by which each difference increased in clearness during the course of each series. The present experiments without these peculiarities and with a very long series give assurance of the justice of their attack on the threshold.

IV.

THE PERCEPTION OF MINIMAL DIFFERENCES.

At the beginning the observer was wholly unused to repeating a judgment of small differences time after time in the manner required by the conditions of such experiments, and her interest could not be maintained at a proper pitch without constant correction and the assurance that each difference was yielding a majority of correct answers. Differences so small that they would not yield a fair majority of right judgments were therefore precluded, and the region just about the point of apparent equality remained unexplored, leaving the possibility that differences less than one-half of one per cent. (the smallest of the series) might be virtually equal to zero. There was the possibility also that the zero point might be seriously displaced by some of the constant tendencies which are known to affect judgments of this kind and in that case the number of correct answers yielded by the difference between 100 and 100.5 would be deceptive because two weights of 100 would not appear equal and would not afford the proper starting point for calculation.

When, therefore, at the end of some five months of practice once or twice daily, the observer ceased to take much interest in the corrections or in the preponderance of right answers, it seemed desirable to introduce cases involving little or no difference and slight negative differences as well.

In the new series the standard was kept at 100 grams and the differences ran by steps of 0.2 gram above and below this. There were fourteen pairs ranging from 97.8 grams to 100.4. No corrections were made, and the experimenter did not know what pair had been given until after judgment was passed by the observer. In all other significant respects the conditions were the same as those of the preceding series.

Since the difference betwen the various time and space orders has no immediate connection with the discussion of the threshold, only one arrangement was used in the series; the standard was always on the right-hand side and lifted first. This corresponds, essentially, to Group P of the preceding series (2d greater and left) for the second *feels* greater the larger part of the time even though it is generally less in actual weight. Needless to say the observer was ignorant of the arrangement of the weights.

The observer was by this time fully aware of her tendency to call the left-hand (second) weight heavier, and she was given to understand that it would appear so somewhat more than half the time. Since it was not possible to predict the force of this constant tendency nor to guarantee its constancy, no attempt was made to get the same number of differences above and below the point of apparent equality. Preliminary exploration gave 98.6 as the weight which would appear equal to 100, and four steps were taken below this to allow a safe margin; but as a matter of fact 98.4 is the weight that balances 100 and only three of the fourteen weights are below this. The second weight was judged heavier than the other 7786 times out of the 14,000 trials.

The results of 1000 judgments on each of the fourteen pairs are given in Table III and fig. 2. Each increment of 0.2 gram produces a corresponding increase in the number of times the second weight is judged heavier than the standard. With the standard preceding and on the right hand, a second weight of 98.4 appears almost equal to it, *i.e.*, either weight is judged to be heavier the same number of times. If however, the second weight is as much as 0.2 gram (two parts in a thousand of the standard) heavier or lighter it does not, in the long run, appear equal. This absolute increment is small, and the relative increment very small. It is not necessary to employ smaller differences in order to show that any difference, no matter how small, is perceptible in the course of a number of trials sufficient to eliminate accidental disturbances. There is no "least perceptible difference"; the threshold has passed the vanishing point.

The threshold is not a point in the scale of differences such

TABLE III—MINIMAL DIFFERENCES.

Number of judgments "2nd greater" in each series of 100 cases.

Series	Amount of Difference in per cent. of Standard of 100 grams −2.2	−2	−1.8	−1.6	−1.4	−1.2	−1	−.8	−.6	−.4	−.2	0	+.2	+.4	Total
1	34	37	38	57	59	54	50	56	51	64	57	64	60	62	743
2	32	42	43	43	46	55	54	49	51	54	58	52	52	63	694
3	41	50	52	45	51	57	54	52	59	49	56	55	67	70	758
4	35	41	43	55	46	58	57	56	57	54	61	59	64	68	754
5	59	46	59	56	57	50	52	56	59	57	69	65	70	67	822
6	50	52	50	49	52	48	61	62	61	69	63	69	77	74	837
7	46	45	50	50	58	56	54	56	57	63	57	64	59	71	786
8	49	49	49	52	54	57	62	58	57	60	61	65	62	64	799
9	47	51	51	51	62	66	58	60	55	59	67	68	64	67	826
10	35	43	51	49	56	52	61	59	58	52	58	63	63	67	767
Average	42.8	45.6	48.6	50.7	54.1	55.3	56.3	56.4	56.5	58.1	60.7	62.4	63.8	67.3	779
m.v.	*7.4*	*3.9*	*4.4*	*3.5*	*4.3*	*3.5*	*3.5*	*2.9*	*2.5*	*4.8*	*3.3*	*4.1*	*4.5*	*2.8*	*35*

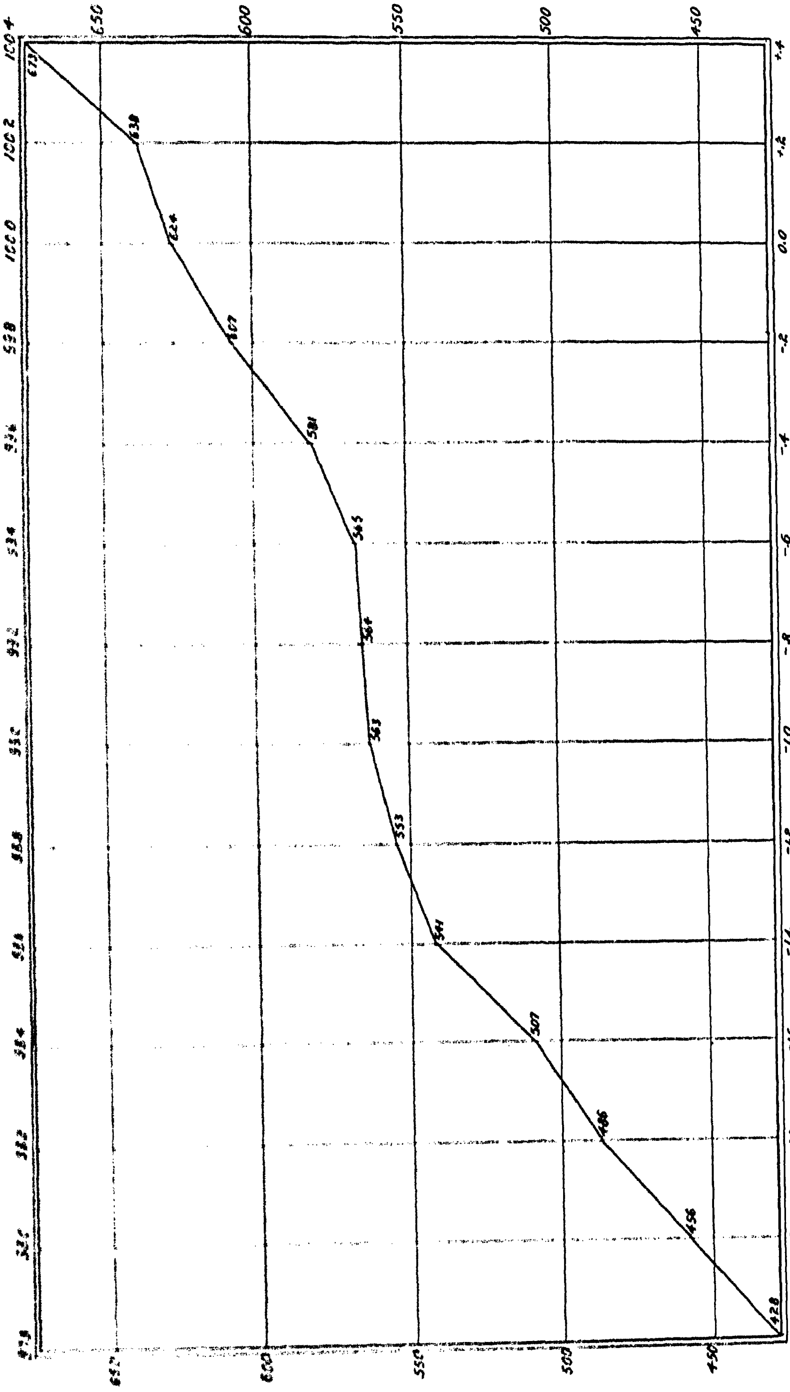


Fig. 2.—The number of judgments "2nd greater" (measured vertically) given per 1000 cases for each amount of difference (measured horizontally) from the standard of 100 grams. Data from Table III.

that a smaller difference will always appear equal to no difference; so much is indicated by these experiments. The previous experiments with larger differences indicate that it is not a point such that larger differences are always perceived correctly. The only conception of the threshold which is compatible with observation is that of a difference which yields a certain proportion of right and wrong answers. In this case the term becomes merely a short expression for a statistical relation with which it has no intrinsic connection, and may become a source of confusion because equally applicable by different persons to different proportions of right and wrong answers.

If it is true, as these experiments seem to prove, that the threshold has no significance beyond the statement of an arbitrary mathematical relation between the frequencies of different judgments, would it not be better to omit the term altogether and retain only the direct statement of the frequencies? If one means to say that 50% of the answers are right, it would seem more simple to state that ratio without cumbering it with the dubious implications of the traditional term "threshold."

If, on the other hand, the threshold can justify its existence in purely psychological terms as the point where differences first *seem* to become perceptible, it will more than ever forfeit its claim to employment in an exact mathematical discussion. Each experimenter who employs it with a real psychological meaning will do well to eschew statistical treatment until he can show for what proportion of right answers he is making it stand and within what limit it actually represents that figure.

The conditions of experimentation usually preclude a correlation of the proportion of right and wrong judgments with the feelings of confidence which determine the subjective threshold of perception. But without a special correlation between them it is not proper to speak of the one as if it had meaning in terms of the other, of the subjective threshold of doubt or certainty as if it implied a normal majority of right judgments, or of the mathematical threshold at 50% as if that figure necessarily marked the division between uncertainty and confidence.

On the whole, much misunderstanding will be avoided by confining the terms limen and threshold to the points marked by some specified degree of confidence, while the more exact measure of sensitivity and discrimination in terms of the proportion of right judgments is limited in statement to the mathematical terms which are proper to it.

V.

UNEQUIVOCAL ANSWERS.

The direct method, as in the present case, *by finding the frequency with which each type of judgment occurs with any amount of difference*, aims to discover whether or not there is any psychological threshold or any point where judgments of a particular kind begin or leave off. This aim is defeated unless the kinds of judgment employed are mutually exclusive. Evasive answers such as "equal," "don't know," or "no difference," permit the observer to avoid forming a judgment in just those cases where it is most important to discover whether or not a correct judgment can be formed. Any method of counting such answers begs the whole question by assuming to know what the observer himself would not tell, namely, how strong an inclination he had to judge one way or the other, that is, how great a probability there was that a certain judgment would have been made if it had been necessary to make one.[9]

That the classes of judgments may be mutually exclusive and that the tables of frequency may have a significance independent of the peculiar coloring that each observer attaches to his words, it is necessary to start with simple alternatives. One form of the method of just noticeable differences meets this condition with the alternative expressions 'difference' or 'no difference.'[10] The method of serial groups[11] also meets the condition in the same terms. If, on the other hand, the presence of a difference is guaranteed, the two directions become simple alternatives (*e.g.*, the second weight is "greater" or "less") under conditions

[9] Urban, *Application of Statistical Methods to Psychophysics*, p. 100, finds that a subject will overlook certain differences large enough to turn his judgment if forced. Cf. Jastrow, *Am. Journ. Psychol.* 1, 1887, and Fullerton and Cattell, *On the Perception of Small Differences.*

[10] Merkel, *Phil. Studien* 4, 1888, p. 262.

[11] Stratton, *Psychol. Rev.* 9, 1902.

which make it possible to repeat the experiment at will with the assurance that the judgments are unequivocal and that every least influence will appear on the one side or the other, no third alternative remaining. Unless such simple alternatives are insisted upon, a certain portion of the cases remain undetermined and the whole distribution is invalidated. The observer can put his own interpretation (and there is no guarantee of his constancy) upon the "equal" or "don't know" class, making it the repository alike of cases where there is a difference of which he is not very sure or which are uncertain in point of direction, and of cases which present no apparent difference; it is therefore uncertain how many of these judgments belong in any one category.[12]

With the conviction that the unequivocal alternative between "this is heavier" and "that is heavier" is the most natural and most psychological basis for the expression of judgments upon the relation between weights, all the preceding experiments were cast in that form. But in order to escape the probable objection that the character of the results is largely dependent upon this arbitrary limitation of the expression of judgment, a series was introduced in which the alternative was of a different sort. In this series there were twenty weights: two each of 99.5, 100, 100.5, 101, 102, 103, and one each of 104, 105, 106, 108, 110, 112, 114, and 116 grams. The 100-gram standard was given first and on the right-hand side, and the observer was so informed. While several of the weights were but little removed in amount from the standard, yet all of them were such as would normally appear, in this position, heavier rather than lighter. The observer was informed

[12] For a vigorous statement of the objections to this procedure see Müller, *Gesichtspunkte*, p. 14; Wreschner, *op. cit.*, p. 30; Merkel, *Phil. Studien* 7, p. 586. In favor of the method cf. Jastrow, *Am. Journ. Psych.* 1, 1887, p. 281; Kraepelin, *Phil. Studien.* 6, 1891, p. 496; Fullerton and Cattell, *Small Differences*, p. 127. A full discussion with references is to be found in Titchener, *Exp. Psychol.* II, part 2, p. 285 ff. It is a question of fact whether or not the observer is disturbed by being forced to judge; most observers find no difficulty in deciding if only they are assured that there is always a difference; the present observer was very seldom compelled to guess. Some experimenters are quite willing to forego "equal" judgments and others to give up "uncertain"; both can be discarded without subjective loss.

that there were cases where the weights were the same and where they were different (always the second heavier) and that she was to decide whether or not in any particular case the difference was present. She designated the two cases "same" and "heavy" referring to the second weight. At first there were some cases in which the *first* (standard) seemed heavier (see cases prefixed by minus sign in Table IV), but these gradually ceased as the observer tended to assimilate all such cases to the "same" category. No corrections were offered and all the conditions were the same as in the preceding series.

The results of this series (600 comparisons of the small differences and 300 of the larger ones, Table IV) show substantial agreement with the other series. Each increase in the amount of difference increases the probability that it will be perceived, *i.e.*, that the two weights will not appear the same. There are lapses with the large differences, as in the first series, but these are not so frequent, possibly because of greater practice and possibly because of the larger proportion of small differences in the series. The results of this series are not so regular as those of Table III (minimal differences) but they are sufficiently regular to show that there is no break between the classes of "same" and "different"; each amount of difference elicits a certain proportion of judgments of either sort. There is no evidence that the choice of a particular kind of alternative is responsible for the steady grading off of the number of judgments from a maximum in one category (such as "left greater" or "same") to a maximum in the opposite category.

This part of the experiment does, however, furnish abundant evidence to justify one in forcing the observer to state in every case which weight is heavier. The category "same" or "no difference" turns out to be unnatural and unsatisfactory to the trained observer. After about 4000 of these comparisons the observer made the remark "those two are exactly the same, and that is the first pair that has been so." This remark is indicative of the whole situation. Starting with the assumption that this series would be less exacting that the other because it did not call

TABLE IV—SAMENESS.

Number of judgments "2nd heavy" in each 100 cases.

	Amount of Difference in per cent. of Standard of 100 grams													
Series	−.5	0	+.5	1	2	3	4	5	6	8	10	12	14	16
1	32	26–8	40–3	44–4	50–4	61–1								
2	33–2	27–7	51–2	48–3	54–2	70	71–2	78–1	87–1	86–2	96	96–1	100	99
3	33	42	50–2	62	52–1	72–1								
4	34	37–1	41	36	45	65	72–1	84	87	97	98	99	100	100
5	27	39	44–1	41	52	64–1								
6	25	26–1	26–1	43	49	52	63	85	83	91	98	100	99	100
Average	30.7	32.8	42	45.7	50.2	64	68.7	82.3	85.7	91.3	97.3	98.3	99.7	99.7
m.v.	*3.1*	*6.5*	*6.3*	*6.2*	*2.3*	*5*								

for so explicit a decision, it soon became evident that it was more exacting on that very account. The quality of sameness rarely or never occurs pure. It is necessary to decide in each instance *how near alike* the two things are and then decide whether that amounts to equality. In other words, one must maintain a mental standard of equality. As one becomes more or less scrupulous this standard is liable to vary; impressions that at one time seemed slightly different now pass for the same. This is evidenced in the present case by the decrease of judgments "first greater" as the observer came to accept these as cases of equality.

Returning now to the series with minimal differences (Table III), we see the other aspect of the same situation. Decisions were there made upon extremely small differences with the utmost rapidity and facility. No matter how small the real difference, there was always a sensible difference sufficient to call forth an immediate judgment.

The results of these experiments should be sufficient answer to the objections advanced by Ebbinghaus and others against the exclusion of judgments of "equal" or "don't know"—objections on the ground that the observer must not be forced or constrained out of the natural channels of expression. As a matter of fact no constraint is involved in the exclusion of these expressions in the case of observers who have had practice in attending to small differences. In the present case the observer gave this introspection in the middle of the minimal difference series: "The weights never seem exactly the same. There is always a difference, but because of the constant tendency to choose the one on the left hand [as heavier] I cannot always be sure." Not over six times in the course of these 14,000 cases was there any outward sign of hesitation. In the face of experience, *a priori* objections in this matter are out of place.

On the other hand, judgments of "don't know" spring from a quite different state of mind and if encouraged are likely to become very frequent. When the difference is slight one must overcome some mental inertia before he can make up his mind, and if allowed to shirk, it is natural for one to lapse into a state

of mild aboulia. No one would maintain that such states of vacillation are desirable in a psychological observer, and it is one of the chief advantages of the procedure under discussion that by prohibiting indecision it abolishes the tendency to vacillate and keeps the observer in a judicial attitude.

The actual array of the minimal difference judgments is a sufficient guarantee that the clear impression of difference is not wholly illusory and that the observer does not guess at random. These judgments are consistently dependent upon the actual differences, and while one often receives a clear impression of difference in the wrong direction yet he may be sure that he will be right in the long run if he trusts to this impression. If the constraint resulting from the necessity for making a choice were sufficient to interfere to any considerable degree with the process of judgment, it would not be possible to get this clear impression of difference and have it trustworthy.

The net result of these experiments and the accompanying introspections is distinctly in favor of insisting in every instance upon a choice, on the assumption that there is always a difference. By doing so every possible factor which can enter into the judgment is included and no violence is done to the observer. He is, in fact, relieved from the rather uncomfortable necessity of making a qualitative judgment. He does not have to have any norm of his own with which to measure his impressions. They are directly measured against each other and the balance struck without ado. By all means let him have his standard of equality if he so desires, but leave him free to change it without risking thereby the unity of the experiment. There is no danger so long as the private standard is kept private and the judgments given in terms which do not involve it.

In order to avoid all danger of a change of attitude and consequent change in the distribution of judgments it is wise to exclude also gradations of judgment.[13] One can never be sure

[13] Cf. Müller, *Gesichtspunkte,* p. 27; also Martin and Müller, *op. cit.,* p. 19.

how the observer draws the line between "greater" and "distinctly greater."

Once assured that there is no ambiguity in the terms of the judgment, and giving up once for all the attempt to answer two or three sorts of questions with one judgment, the tables of frequency begin to take on real significance. A majority of correct judgments represents an appreciation of the true relations of things while a larger majority means a more frequent appreciation. Without any reference to subjective feelings of doubt or assurance, it is possible to say whether there is any difference so small that it is never recognized or any so large that it is always appreciated. Nor is it necessary to consider the sensations which lie back of the judgment. To be sure, every comparison involves logically the perception of each stimulus separately, and a distortion of one or the other of these may be responsible for erroneous judgments. Such distortions, however, when allowance is made for constant factors, become themselves a matter of chance and fall into that half of the total number of cases which is conceded to the influence of circumstances other than the prescribed conditions of the experiment. The judgment itself is not about either one of the stimuli but about the relation between them, and it is true psychologically that this relation can, and often does, make itself felt without conscious reference to the separate terms.

VI.

TIME AND SPACE FACTORS.

The relative importance of the space and time factors in the series (Table II) which illustrate them is uncertain by the nature of the case, for the time order is constantly being reversed and is unknown to the observer, while the spatial arrangement remains the same for whole groups of experiments and is known

TABLE V—CONSTANT FACTORS.

The average number of correct judgments for each group, taking several amounts of difference together.

Group	Average of Series	Standard in Grams	2nd Weight in Relation to 1st	Differences 1–6	7–12	13–18	1–18
A	1a–5a	100	greater, left	35.7	46.6	49.5	43.9
B	6a–10a	50	greater, left	32.1	44	48.6	41.6
C	1b–5b	100	less, left	33.4	42.1	47.1	40.9
D	6b–10b	50	less, left	33.5	44	47.7	41.7
E	1–5 (a + b)	100	left	69.1	88.7	96.6	84.8
F	6–10 (a + b)	50	left	65.6	88	96.3	83.3
G	9 (a + b)	50	left	65.5	88.3	98	83.9
H	5 (a + b)	100	left	67	87.5	95.5	83.3
I	21 (a + b)	150	left	75.2	95.2	99.3	89.9
J	11 (a + b)	50	right	64.1	85.3	97.2	82.2
K	16 (a + b)	100	right	67.3	90.7	98.5	85.5
L	22 (a + b)	150	right	71.1	92.3	98.2	87.2
M	9, 11 (a + b)	50	all orders	64.8	86.8	97.6	83.1
N	5, 16 (a + b)	100	all orders	68	89.1	97	84.7
O	21, 22 (a + b)	150	all orders	73.2	93.7	98.7	88.5
P	1–5, 17, 19 (a)	100	greater, left	35.7	46.9	49.5	44
Q	1–5, 17, 19 (b)	100	less, left	33.6	43.4	47.7	41.6
R	1–5, 17, 19 (a + b)	100	left	69.3	90.3	97.2	85.6
S	12–16, 18, 20 (a)	100	greater, right	34.5	46.8	49.7	43.7
T	12–16, 18, 20 (b)	100	less, right	36.1	45.5	49.3	43.6
U	12–16, 18, 20 (a + b)	100	right	70.6	92.3	99	87.3
V	1–5, 12–20 (a + b)	100	all orders	70	91.3	98.1	86.5

to the observer.[14] There are more right answers when the second weight is greater, rather than less (Table V, Groups P, S),[15] or, in other words, when the standard comes first. There are more when the first weight is to the right rather than the left (Groups V, R). The most favorable combination is, second weight greater and left (P); the worst is, second weight less and left (Q); these are compared with each other in the right-hand part of fig. 1. When the second weight is to the right, the time order has little effect; when the second weight is the greater the space order makes little difference.

These facts cannot be accounted for on the basis of Fechnerian errors which have a constant effect in each order equivalent to an increment of weight. They can be accounted for only in part by assuming a constant tendency in favor of the arrangement with standard first. Besides this there seems to be a similar constant tendency operating in favor of the arrangement with standard to the right, so that the apparent difference is generally increased by proceeding *away from* the standard and also by proceeding *toward the left, i.e.,* in the direction of greater flexion of the arm.

In Table VI the data of the long series (P-V, Table II) of 1400 cases for each difference are worked over on the basis of the probability integral. From the number of right cases given for each amount of difference the amount of difference is calculated that would give 75% of right judgments on the assumption of an ideal distribution of the errors by chance.[16] This calculation

[14] The space error can be eliminated easily, but only at a considerable expense of time. The speed of 245 comparisons per hour mentioned by Urban (*op. cit.*, p. 11) was only got with the assistance of a third person and by prearrangement of the series.

[15] This may be attributed either to a simple time error (the preceding stimulus appearing weaker as compared with the more recent) or to the general tendency noted by Martin and Müller (p. 64) to increase the number of right judgments when the standard comes first.

[16] The calculation is as follows: Take the number of right judgments per cent. given on any amount of difference. Look this up in Fechner's fundamental table and divide the corresponding T = hD entry by .4769. This gives Fullerton and Cattell's table. Then divide the amount of difference by this figure, and the result is the amount of difference which would yield 75% of right judgments.

puts in more objective form (though with more theoretical assumptions) the conclusions of Table V regarding the arrangement giving the greatest number of right answers. It shows that the great inferiority of Group Q (second less, left) is due to an excess of wrong judgments on the larger differences as compared with the other groups. (See right-hand part of fig. 1.) It is to be expected that the "constant tendencies" will be more effective on the larger differences for then the one weight is least like the average[17] and Group Q has the disadvantage of both constant tendencies.

TABLE VI.

The amount of difference necessary to yield 75 per cent. of correct judgments, calculated from the number of correct judgments given on each amount of difference for the several groups of Table II.

Amount of difference in per cent. of Standard	P 100 g 2nd greater left	Q 100 g 2nd less left	R 100 g 2nd left P+Q	S 100 g 2nd greater right	T 100 g 2nd less right	U 100 g 2nd right S+T	V 100 g all orders R+U	B 50 g 2nd greater left	D 50 g 2nd less left	F 50 g 2nd left B+D
1	3.73	4.38	4.03	6.66	3.24	4.47	**4.24**	9.80	5.10	6.66
2	4.52	4.70	4.60	6.50	3.57	4.57	**4.61**	9.62	4.44	6.02
3	3.74	5.12	4.35	3.59	3.63	3.64	**3.95**	6.02	6.67	6.32
4	3.67	4.65	4.12	4.65	4.97	4.81	**4.44**	4.04	6.08	4.88
5	4.22	4.75	4.48	3.88	3.60	3.72	**4.08**	6.70	6.04	6.35
6	4.18	4.45	4.31	5.42	4.28	4.80	**4.56**	6.32	4.65	5.37
7	4.34	5.47	4.89	4.08	4.37	4.22	**4.53**	4.92	4.76	4.84
8	3.81	5.83	4.74	4.32	4.95	4.63	**4.69**	4.83	4.59	4.71
9	4.28	5.80	5.01	4.15	5.17	4.65	**4.84**	6.33	5.90	6.12
10	3.84	5.26	4.56	4.15	4.37	4.26	**4.42**	5.44	5.53	5.50
11	3.34	5.25	4.40	3.51	4.39	4.00	**4.22**	5.17	5.38	5.29
12	4.15	6.37	5.30	3.94	4.55	4.27	**4.82**	5.42	6.07	5.77
13	4.33	6.31	5.39	4.26	4.49	4.03	**4.93**	5.13	6.72	5.94
14	3.76	6.13	5.16	3.76	4.47	4.15	**4.71**	5.67	5.67	5.67
15	4.79	5.53	5.18	4.03	4.79	4.45	**4.85**	5.09	6.35	5.77
16	4.75	6.22	5.57	4.29	3.75	4.07	**4.98**	5.43	5.43	5.43
17	3.99	5.88	5.17	......	5.17	4.67	**4.98**	6.18	5.98	6.09
18	......	6.21	5.40	......	4.22	3.93	**4.94**	5.04	6.11	5.63
Median	*4.15*	*5.50*	*4.82*	*4.15*	*4.43*	*4.27*	**4.65**	*5.43*	*5.78*	*5.65*

This same Group Q falls farthest away from the chance distribution, while Group P gives the nearest approximation (of those with a single time and space arrangement) to such a distribution, its excess of right judgments being pretty evenly

17 Martin and Müller, p. 44, show this to be true in the *time* aspect at least.

distributed over the various differences.[18] Group B is similar to P in these respects.

The combined group (V) is a regular distribution but with more errors on the large differences than are to be expected. From the point of view of the so-called "normal" distribution it would appear that the positive constant tendencies of judgment represent the normal as well as the maximum efficiency, while the absence of those favorable or normal conditions causes an excess of errors on the larger differences. In other words, unfavorable arrangements in time or space order have the same effect in producing lapses of attention that we would expect to find as the result of distraction; the positive general tendencies are not then anomalous but represent the absence of distraction.

The gradual relative increase in the number of errors as the amount of difference increases, which appears in the less favored groups and, as a result of their influence, in the combined Group V, is due to the greater force of the constant tendencies with the larger differences. The absence of these larger errors at once assures a greater number of right answers for the whole group and reduces the distribution of the remaining errors to the form given by the conventional error curve. In this case the special negative constant tendencies increase in force with the magnitude of the difference, and in the combined Group V effect a departure from the normal distribution given by the more favored groups which are free from their influence.

The results from very small differences indicate that little can be said about constant errors on the basis of the comparison of equal or nearly equal weights. The determinations are often inconsistent with the larger differences of the same group and inconsistent from group to group. For example, Group S

[18] The comparison can be made in Table VI or on fig. 1 where each of the horizontal lines is at the level of the number of correct answers, according to the ideal curve at the left, which each difference ought to yield if a difference of 4.65% (the median value of Group V, Table VI) yields 75 correct answers per hundred. The plotting of the ideal curve is as follows: The entry under hD in Fechner's fundamental table opposite 75% is .4769; dividing this by D = 4.65 gives h = .103. Then each D is multiplied by this value of h and the per cent. corresponding to the product hD taken from the table.

(Table II) gives less right answers than the other groups for differences of 0.5, 1 and 2 per cent., but this tendency is not borne out by the larger differences. Group Q, which starts positive, ends with a negative error.

The series composed exclusively of minimal differences, Table III, or fig. 2, shows a constant error very much larger than that in the corresponding portion of Table II (Group R). The difference of —1.2 gram gives as many judgments of "second (left) greater" as + 0.5 gram does in Group R, while + 0.4 gram gives almost as many as + 3 grams. This phenomenon can be accounted for in part by the higher attention demanded by the minimal series, and in part by the different impression gained of the general average weight of the group and the consequent change of attitude toward the standard weight which, in the one case, is considerably lighter than the average, and in the other heavier. But making full allowance for these factors it is plain that the consideration of very small differences complicates the study of the constant tendencies very seriously.

Another fact to be noted in this connection is the skew in the distribution of the judgments on minimal differences (fig. 2). The distinct plateau at weights 98.8 to 99.4 with the consequent displacement of the two ends of the curve indicates the presence of a strong unknown disturbing factor. It is very probable that the constant tendency is not simple, but complicated as has been suggested, being made up of several tendencies working more or less at cross purposes. Or, it may be that the amount of the constant tendency, varying as it obviously does, is more frequent at two distinct modal points; there is no reason why we should expect it to be always the same for the same person any more than for different persons, and no reason why it should not oscillate between two quite distinct values.

At any rate, the fact that the judgments do not tend consistently to group themselves about a single value of the constant error as measured by the point at which the weights are apparently the same (either one called heavier half the time) is another indication of the inadequacy of this measure of the constant tendencies.

Though each time and space arrangement has its characteristic curve of distribution, it is not necessarily true that the combined series is not significant. Groups B and D (Table II) with the lighter weights do not differ from each other so much as P and Q (the corresponding groups with 100 grams), and they do not show the same relation to the ideal distribution. With respect to Weber's law or similar questions, it would be fairer to compare the combined number of correct judgments for each standard rather than to compare the separate time and space orders. The method of combining the partial groups must depend upon the special requirements of the particular experiment; the simple combination given in the tables has the advantage of preserving the actual experimental array of figures, unconfused by theoretical considerations concerning the type of distribution.[19]

[19] G. F. Lipps, *Arch. f. d. gesamte Psychol.* 3, 1904, emphasizes the danger of assuming any particular form of distribution. Compare also the work of Urban and of Spearman referred to above, and Wundt, *Physiol. Psych.*, 1908, I, p. 613.

VII.

THE JUDGMENT OF DIFFERENCE DEPENDS UPON THE FORM OF EXPRESSION.

When the preceding experiments had shown that the judgment of difference is conditioned by other factors besides the physical difference between the stimuli, it seemed wise to enquire to what extent it is conditioned by the mere form in which it is expressed.

Two questions in immediate relation with the preceding work presented themselves: the relation of judgments of "same" to those of "greater or less" as measured by their distribution over the same objective differences; and the relation of "greater" and "less" judgments to each other when expressed in different modes. In either case the question is whether the same objective relations between the weights will appear different (in terms of frequency) when judged in series which are alike save for the mode in which the relationship is expressed.

Table VII presents the results of an attempt to isolate these two factors. Three groups of nineteen pairs apiece, designated as *which?*, *second?*, and *the same?*, were run along together in such a way that each group followed itself and each of the other groups the same number of times but in haphazard order.

In the *which?* group the observer was instructed to "indicate which weight is the heavier," and she did so as in all the previous experiments by pushing forward that weight. These results were recorded as "left heavier," or "right heavier," but the observer did not think of the weights in those terms nor as "first and second," but rather as "this and that" in terms of the motor localization of the positions. In the *second?* group the question was put, "Is the second heavy or light (in comparison with the first)?" and the judgment was given orally "heavy" or "light."

TABLE VII—THE FORM OF THE JUDGMENT.

The number of answers out of 200 cases for each amount of difference with each of the three types of expression.

		Differences from the Standard (preceding on the right) of 100 grams.																		
		−18	−16	−14	−12	−10	−8	−7	−6	−5	−4	−3	−2	−1	0	+2	+4	+6	+8	+10
Which?	Number of judgments "2nd less"; motor →									98	90	63	42	44	23	15	9	5	1	1
	← Number of Judgments "2nd greater"; motor.	0	1	7	14	31	65	68	73	102										
Second?	Number of judgments "2nd less"; oral →										98	74	70	39	34	13	6	3	1	0
	← Number of judgments "2nd greater"; oral.	1	0	3	8	23	42	53	80	80	102									
Same?		0	6	9	25	36	74	79	97	96	96	103	106	111	86	48	40	20	6	5

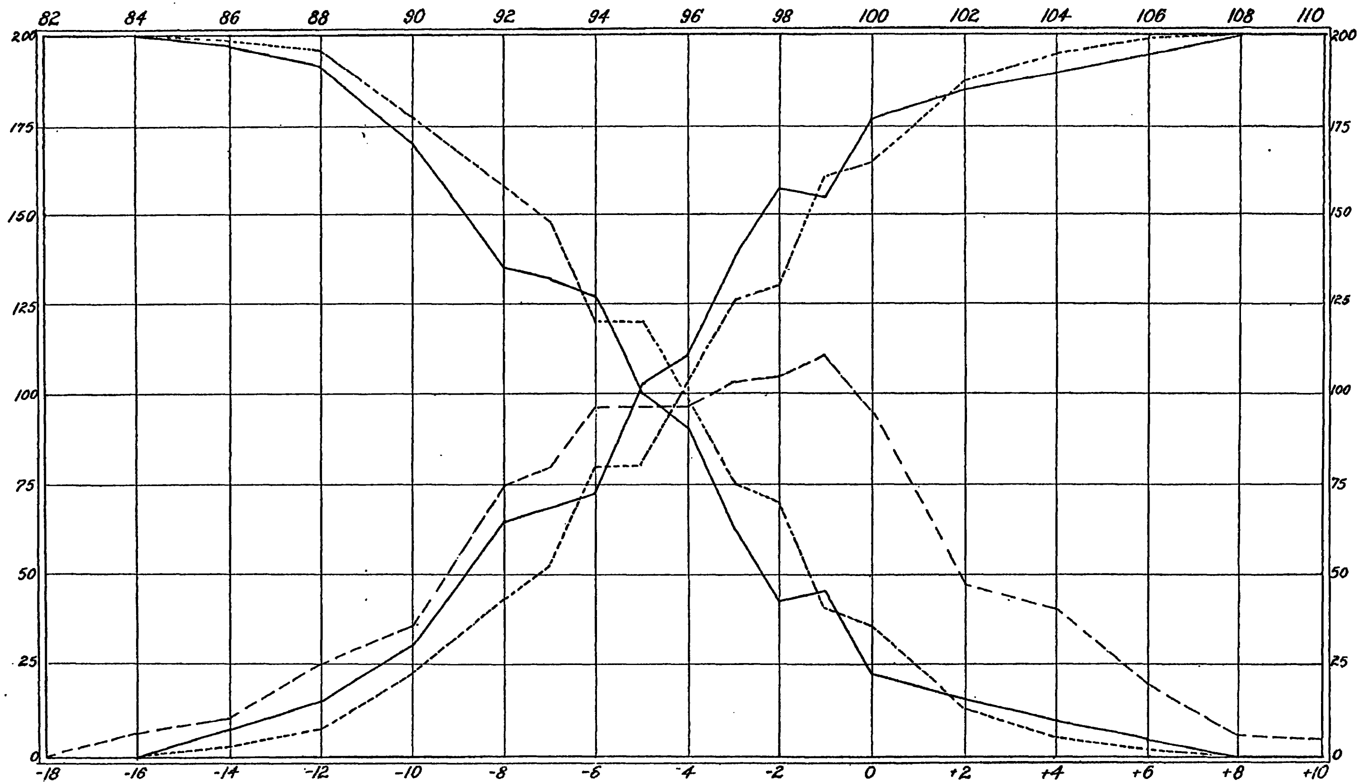


Fig. 3.—Solid line: motor answers in *which?* series. Dotted line: oral answers in *second?* series. From the left up: the number of judgments "2nd greater" for each amount of difference from the 100 gram standard. From the right up: the number of judgments "2nd less." Dash line: the number of judgments "same." Data from Table VII.

In the third group the question was put, "Are they the same (or nearly the same)?" and the judgment was again in oral terms "same" but if they did not appear the same nothing was said.

The series of weights was arranged to fit the requirements of this experiment. By trial it was found that there would be about as many judgments "second greater" as "second less" if the same number of weights were taken above and below 96 grams with the standard (preceding) of 100 grams. The weights ran by steps of 2 grams from 82 to 92 and by 1 gram from 92 to 100, then by 2 grams from 100 to 110, making nineteen members in the series with the center at 96. In each group 200 judgments were recorded for each amount of difference.

The comparison of these groups involves possibly some slight errors. The *second?* group was entirely new while there had been practice with the others. Even though the groups come in irregular order an equal number of times, it is possible that one may affect another more strongly than it is affected in turn. The *same?* group still seemed hard to the observer despite the obvious opportunity it affords for "soldiering." The effect of this group and of the preceding month's practice with a similar test (Table IV) was an occasional occurrence of "same" judgments in the other groups, particularly in the one with oral judgments (*second?*); these were discarded and repeated; their number was not large. Very rarely there was an incursion of one of the other groups into the *same?* group. These were ignored. Very rarely there was some confusion or absent-mindedness as to which group was under way, but as a rule the observer stuck to one set of categories consistently for the nineteen judgments and was then ready for a change to another set of categories or for the repetition of the same group as the case might be.

The common point in these groups is to be sought not so much in the number of correct answers which they respectively yield as in the amount of difference which appears equal, in each one, to zero. In other words, the effect which is to be expected from the influence of different types of judgment is one which can be measured directly by a change in the constant error.

The constant tendencies connected with temporal and spatial order have already been shown to be as inconstant as are most "constant" tendencies. There is some indication that they rest largely on mental habit and a certain unreasoned and unconscious tendency to prefer one form of expression to another. Their constancy depends in large measure on keeping the conditions constant with regard to the number and magnitude of the members of the series (compare Tables II and III), and upon the amount to which the preferred form of expression interferes with the normal course of judgment.

The comparison of these three groups in which the only difference is the form in which the judgment is framed goes far toward proving that the constant tendencies are due to the attitude and bias of the observer and not to fatigue or a physiological change of any sort. Far too much attention has been paid heretofore to obscure or hypothetical physiological factors in the production of constant illusions of all sorts and far too little heed has been paid to purely mental factors, the central tendencies and habits which the person himself brings to bear upon the situation. The mere fact that individuals have opposite constant errors in exercises which involve little muscular force, such as reproducing lines or even recognizing grays, ought to have suggested the possibility of a fortuitous central bent independent of the immediate task. It is quite possible that some individuals always prefer the judgment in the form "greater" to that in the form "less" even when dealing with opposite aspects of the same proposition, as for instance "Is the second greater than the first or is the first less than the second." But in the present instance the difference between the groups is more complicated. In the *which?* group the responses are in motor terms in which the movement is an habitual response in the same member which receives the stimulus. In the *second?* group the responses are in verbal terms which may or may not be a translation of the idea accompanying the motor response in the other set and which may themselves be subject to a motor speech-habit such that one of the words in question is more apt to occur than

the other independently of the idea in mind. There is also a conscious attitude in the *second?* group by which it is distinguished from the other. The attention is directed more exclusively to the second weight; the first is taken for granted and is readily fused in the general impression of the average weight; and the absolute heaviness or lightness of the second weight is relied upon to a large extent in forming the judgment. In the *second?* group, also, the observer's attention is attracted to the known time error which makes the second weight seem heavier than it should, and there is a more or less conscious tendency to compensate for this error.

But whatever the precise differences between the groups may be, it is evident that they are all of the nature of changes in the mental "set" or bias of the observer. There is now no difference in the number or amount of the difference and certainly no difference of any kind in the objective presentation. If any difference between the groups appears it must be due wholly to the interference of the observer's own mental machinery. The results which do appear under these circumstances are sufficiently clear.

For purposes of comparison it may serve to take the *same?* group as the starting point. The amount of difference in that group which yields the maximum number of judgments "same" has a peculiar claim. That these are the two weights which seem most nearly alike in this time and space order (standard first, and right) there can be little doubt. But as has been said before the quality of *sameness* does not occur pure. The observer was instructed, after what had been learned from the preceding attempt and failure to get clear impressions of sameness, to indicate the weights which were "the same or nearly the same" as the standard. This judgment is an approximation only, but there is every reason to suppose that when a weight of 99 grams appears equal to 100 more times than does any other weight it is the one which appears most nearly equal to 100.

It will be remembered that the observer had learned in the preceding set of experiments (Table IV) to assimilate a certain number of "second lighter" cases to the category of "same"

cases. This effect may have been carried over, and in fact the observer believes that it was carried over, to the present group. If so, the apparent zero point is displaced toward the lower end of the scale. Even so, this apparent or subjective zero point is as good a starting point as any other. In any case it would probably be displaced one way or the other; when no perfectly clear impression of sameness appears it is far from probable that the judgment of sameness will attach with perfect impartiality to the cases of "slightly greater" and "slightly less."

Notwithstanding the effect of training which would naturally tend to increase the constant error as measured by the maximum number of "same" judgments, it is clear from Table VII and fig. 3 that the constant error is not nearly so great in this series as in the other two with which it is to be compared. The maximum falls at 99 as compared with 95 and 96 (the indifference points of the other curves). This group is differentiated from the others also by the fact that the number of "same" judgments on all the differences from — 4 to + 10 is very greatly in excess of the number of errors yielded by those differences in the other groups. Judgments of "same" occur when no errors are made in the other groups and they are frequent when the probability of an error in the other groups is very low.

The introduction of "same" judgments into a series surely obscures the results and precludes the possibility of finding what the actual distribution of "greater" and "less" judgments would be. In the present case one-half of the whole number of "same" judgments fall on differences greater than — 4 grams and the maximum number falls on the difference of — 1 gram; whereas the half of the wrong answers fall on — 5 and — 4 respectively in the other series, and the indifference points are also at — 5 and — 4. Even making allowance for the fact that the curve of "same" judgments is skewed, the evidence is clear that a judgment of "same" is more likely to correspond with a judgment of "second greater" than with one of "second less." A distribution of the "same" judgments into cases of "greater" and "less"

on the basis of a zero point at either — 1 or — 4 could not fail to do violence to the facts as determined by trial.

Add to this difficulty in distributing the "same" cases the other fact that they occur when the observer would not otherwise be likely to make any mistakes, and it will be evident that a series in which they occur and are encouraged is not suitable for accurate analysis.[20] A further weakness in this group is the skew in its distribution by which the point (99) where the greatest number of "same" judgments is recorded is far removed from the point (96.5) on either side of which half of them fall. The "same" judgments do not even afford a reliable measure of the constant error or point of apparent equality.

The two remaining groups (motor, *which?* and verbal, *second?*) differ from each other almost as clearly as the *same?* group differs from them. The curves of fig. 3 show that with only two exceptions the number of "second greater" judgments given verbally is less than the number indicated by motor sign for all the differences from the lower end of the scale up to + 2. From a difference of + 2 up to the upper end of the scale verbal "second greater" judgments are more frequent than the motor. The two exceptions have every indication in the plot of being accidental. The inference from these data is that except for large positive differences this observer will give more judgments "second greater" and fewer "second less" when the judgment is expressed by indicating the heavier weight than when it is expressed by saying whether the second weight is heavy or light in comparison with the first. It is a normal consequence of this general tendency that the difference (— 5 grams) which gives an equal number of "second greater" and "second less" judgments in the motor series gives a majority of "second less" judgments in the verbal series. The displacement happens to amount to

[20] Wundt, *Physiol. Psychol.*, 1908, vol. 1, p. 614, shows the extremely irregular behavior of the "same" judgment, *e.g.*, in the work of Keller, before mentioned. The use of these expressions is bound to introduce real (not merely mathematical, cf. Titchener, *Exp. Psychol.* 2, II, p. 290) difficulties in statistical treatment, and can only be justified by special application of the results.

exactly one gram, so that the difference of —4 grams in the verbal series is its indifference point.

It so happens that both the motor and verbal groups give symmetrical distributions so that the sum of the judgments "second greater" below the indifference point is nearly equal in either one to the sum of the judgments "second less" above. That is, one-half of the errors lie on either side of the point which gives the maximum number of errors. This distribution is in contrast with the skew in the *same?* series.

Since both the *which?* and *second?* groups are of this symmetrical form, the difference between them is the more obvious. The difference between the two methods of judging is constant up to and beyond the point where the weights are objectively equal. With actual plus differences the tendency is reversed, but the whole number of errors is so small that it makes little impression upon the aggregate number of judgments of each type.

Total of all judgments "2nd greater" for 19 differences; Motor	2068
Total of all judgments "2nd greater" for 19 differences; Verbal	1952
Half of all the judgments for either group	1900

The mere change in the expression of the judgment is evidently responsible for the large preponderance of "second greater" judgments in the motor (*which?*) group.

If, instead of taking the whole number of answers of any one sort, the number be taken of those which are wrong from the point of view of the indifference point the same relation is discovered.

Motor; number of errors (2nd greater) below 95 grams	259
Motor; number of errors (2nd less) above 95 grams	293
Motor; total number of errors	552
Verbal; number of errors (2nd greater) below 96 grams	290
Verbal; number of errors (2nd less) above 96 grams	240
Verbal; total number of errors	530

The relation expressed above would not be seriously affected by including the steps which are omitted between 82 and 92, on

one side, and 100 and 110 on the other. Rather it would be made stronger, if anything, because the tendency is stronger on the greater differences than in the middle range. The differences tried cover practically the whole range within which errors occur. It is evident therefore that there is an advantage for the *second?* (verbal) group over the *which?* (motor) group in regard to the total number of errors.

The group which appears to better advantage here is that which adopts the procedure recommended by Müller and Schumann. It has less errors in all and a less dispersion of errors toward the larger differences. It also shows a less exaggerated constant error. So far as the small number of cases warrants any conclusion, it seems also to present a more symmetrical distribution of plus and minus errors and to have greater regularity. The method adopted in the preceding experiments suffers by comparison in all these respects notwithstanding the advantage it should have had from long use in the hands of this observer. This procedure differs from that of Fechner only by substituting the motor sign for a verbal judgment. There is no evidence to show that this motor feature rather than the pure Fechnerian element is responsible for the increase in constant error, together with general raggedness, which this group shows in comparison with the other.

Besides discrediting the use of ambiguous terms by which the observer is enabled to evade the effort of deciding the question presented in the comparison of stimuli, this set of experiments brings out the importance of all the terms used in the formation of a judgment. The results leave no doubt that a difference in the framing of two propositions which are precisely equivalent logically will be a governing factor in making a comparison. Evidently no comparison is complete with the mere apprehension of the presented stimuli. These are apprehended in the light of other stimuli which have gone before, but even then the analysis is not complete without taking account of what the observer has to do in the matter. Even the very slightest differences in the task which he has to perform seem to govern to some extent his decisions.

To speak of the "perception of difference" in such a case is to obscure some of the factors in the actual situation. The difference is not merely perceived. The process of comparison involves the active operation of the mind in the expression of a judgment upon the situation in which the difference is only one factor. When this difference is acted upon through one set of categories and with one mental set it occasions one definite reaction, while if it is taken into another set of categories it goes through different mental machinery and comes out different. If it were possible to catch an instantaneous view of the two experimental groups under consideration, there is no doubt that a weight of 95.5 grams would be sensibly lighter than 100 in the one and heavier in the other. The stimuli to be compared are identical and the difference involved is not conceivably other than identical. Moreover the logical relations of the terms are equivalent. And yet this difference comes out plus in one group and minus in the other. In the instantaneous view it is judged to be sensibly other; to be two distinct differences.

There are practical objections to having the same thing be different from itself, and if it does appear so it is generally worth while to locate the mental defect which brings it to pass. In the present case the defect seems to be more deep-seated than a mere inability to perceive accurately what is presented. It seems rather to be a systematic distortion which may extend to any subject-matter whatever. The terms of judgment under consideration are of very general use; they can be applied to anything which is quantitatively greater or less than another of the same kind. If it be true that the mind will more readily give expression to "greater" than to "less," the fault is certainly not in the perception of the particular difference but rather in the mind's attitude toward all differences.

Such a defect would permeate all quantitative judgments, and would, in fact, be a defect of judgment itself. There seems to be evidence that some of the abnormalities observed in the comparison of weights are traceable to such subtle eccentricities in the machinery by which all judgments of difference, in any material, are expressed.

VIII.

THE JUDGMENT OF DIFFERENCE IS CENTRALLY CONDITIONED.

Throughout this paper as well as in the title the term *judgment of difference* has been preferred to the more common expression *perception of difference.* The latter fails to express the active operation of the mind in the discrimination of objects. If two weights had a difference which was always sensibly the same or nearly the same, it might be legitimate to speak of perceiving that difference. But in fact the judgment of difference is by no means so simple. Given a certain physical difference between two weights, one can perceive it more or less clearly, but one is also liable to perceive in its place *another* difference which is just the opposite of the true physical difference. Not only does the difference to be perceived possess this peculiar faculty of turning its coat, but it can change its whole aspect in other ways quite as alien to the general character of perceptible things. It is subject, as has been shown, to a startling contrast effect. Surrounded by larger differences it becomes almost imperceptible, but if put among smaller differences it presents a bold face. This effect is not, however, produced by any change in the objective field, such as that causing sensory adaptation or that which brings out color contrast. The objective conditions are precisely the same every time a pair of weights is compared. The only difference is the fact that previous comparisons have taken place. The contrast effect depends not upon the objective conditions but upon the attitude of the observer and the direction of his attention.

The difference is in no case simply perceived. It is perceived in view of other facts which have no inherent connection with it. If it were perceived as ordinary objects are perceived, it would maintain an identity of its own and, subject to more or less

fluctuation, it would always be recognized as itself. Every object that is perceived has an identity of its own and accordingly has a place in systematized experience into which it fits. Its identity gives it its relations with the other objects which we perceive and know in the world of daily activity. It is in this respect, particularly, that the perception of difference shows that it is of another sort from the perception of other things.

Within any one of the preceding experimental groups where the conditions remain fairly constant each amount of difference does maintain its identity. But as soon as we pass to another group in which the conditions are somewhat different the identity disappears. If, for example, we take 75% of right answers to be the mark of a difference which is "just perceptible" then we find in Group R of Table II that a difference of between 4 and 5 grams will be just noticeable or will be a "threshold value," as it is said. But turning to Table III, we find it unreasonable to suppose that the threshold value is far beyond 1 gram. In Table IV, again, this "threshold" difference is at about 2 grams, and in Table VII it is pushed down to between —2 and —4 grams.

That these differences between the various groups are genuine and do not represent merely a general change in the observer such as to cause a large shift in the constant error, may be seen from the final series (23) of Table I. This series was taken immediately after those of Table VII which show a threshold of —3% and it was separated from the last of the same sort (22) by some 30,000 cases; yet, with allowance for some general improvement with practice, it shows substantial agreement with the preceding series in its group and its threshold is over + 4% as in the others. There had been no change of consequence in the constant error for this set of weights during the interval.

This dependence of the constant error upon the range of differences comprising the experimental series seems to be closely connected with the behavior of the "indifference point" in the method of average error. Hollingworth[21] has shown that the

[21] *Archives of Psychology,* No. 13, 1909, p. 31.

latter "so far as it occurs in our spatial judgments, and in our temporal judgments so far as they are a function of the extent of movement, is of purely central origin, and that its position depends entirely upon the range or limits of the magnitude in question." The indifference point in the error methods corresponds closely to the point in the method of constant differences where errors are most likely to occur in half the number of judgments. It is not unlikely that a shift in the indifference point in our method would be influenced by conditions which cause a shift in the indifference point in the other method.

The position of the threshold, however it is defined, is immediately dependent upon the indifference point in the method of constant differences, that is, upon the conditions which make the differences equal to zero. This point is certainly dependent upon the range of differences covered by the series. In the error methods the point where the error is as likely to be plus as minus is determined certainly in part if not wholly by the same conditions.

This condition has been so very generally ignored that its influence may be regarded askance, but its appearance now in both methods of procedure cannot be overlooked. It is perhaps too much to say that the indifference point, and with it the threshold, can be shifted at will by simply changing the magnitudes of the largest or smallest stimuli in the series, yet this is to a large extent true.

It may be unwise at present to assert that the change in the magnitudes works wholly as a change in the central elements in the judgment, but at this writing there is no evidence that the total situation is changed in any other than its central factors.

The extension of the series toward one extreme or the other should not logically interfere with the proper perception of a magnitude in the midst of the series. But it does so; and it is not clear that it does so in any other way than by disturbing the equilibrium of the observer's mental attitude toward all the individual magnitudes making up the complex system of the series. Under these circumstances the magnitude in question is distorted

by the mind and misrepresented in reproduction. Under such conditions the judgment of differences between stimuli is affected as well as the judgment of the individual stimuli. So it is that in the different groups when the set of weights is not the same (the standard being constant) the same physical difference is not perceived as the same or even as similar.

The change in the total group of weights which make the setting of any single pair may be likened to the change in total illumination which causes red to fade into black with the advance of evening. But in this case there is no advance of evening and nothing corresponding to it; any given pair of weights is compared under such circumstances that the physical stimuli are in all respects identical. The case is more like that in which an orange object is perceived now as red and now as yellow. It is red in a universe of yellows and yellow in a universe of reds. The last case is one which is thoroughly familiar and in which the perceptual factor is clearly isolable. When an effort is made to perceive what the color is, it must be abstracted from the particular surroundings which make it appear to be red or yellow. It is perceived truly enough to be a color, and that too of the red end of the spectrum, but if a closer identification is necessary it must be perceived as an orange in order to be perceived at all in the proper sense of the word. One may say that under the circumstances it looks like a yellow but one fails to perceive it properly unless he is aware that it *is* an orange even though it looks yellow. The failure to interpret the sensation in such a way that it will get properly entered in the total range of normal experiences—in this case in the range of color qualities—is a failure of perception.

But aside from the perceptual element by which the object is more or less perfectly identified there is the element of comparison within the group. This comparison may be quite unconscious, but its result is obvious in the ultimate ranking of the members of the group with respect to their common quality. In the case of the difference between weights the operation of this process of ranking cannot be gainsaid. In all the experi-

mental groups, no matter what the conditions, the largest difference is found at the top of the scale and the smallest at the bottom, with the others graded in between. In other words, the relative magnitudes of the differences are always, in the long run, judged correctly.

But this judgment and the consequent ranking do not depend on a correct perception of the actual differences involved. In fact the record shows that the process of judging the relative magnitudes of the differences interferes to a marked degree with the process of perceiving or recognizing the true differences. Because the difference of about 0.5 gram is the largest in one series, the smallest in another, and almost the smallest in another, it is interpreted in the several cases as a fairly clear, a barely perceptible, or a "threshold" difference. Its appearance is so altered by the value given it in its own group that it cannot be recognized in another group. It is not properly perceived but it is none the less well judged in relation to its fellows. The more familiar case of color is analogous. The fact that the one in question is ranked at the red end of one scale and the yellow end of another tends to break down its identity and to interfere with its proper perception. So, too, a man who is tall in one company and short in another maintains his identity through the possession of other qualities besides that in question, but if he were to be identified by his height alone he could escape unperceived simply by a judicious choice of companions. Few people could perceive his true height in the terms of an arbitrary conventional standard. Most people would merely judge his height in relative terms, just as they judge the amount of difference between weights which have no accidental mark of identification.

IX.

WEBER'S LAW.

Upon the notion of the threshold, or point of just perceptible difference, hangs Weber's law in its usual formulation.[22] But the statement of the facts embraced under Weber's law and what they mean ought really to go much farther. There is no propriety in saying that a certain relative difference which can just be perceived on one standard can just be perceived on double that standard, unless the term "just perceived" means exactly the same thing in both parts of the proposition. If, now, the proposition is asserted to be true when "just perceived" is taken to mean "perceived 75% of the time," there is no reason why it should not be quite as true when "just perceived" is taken to mean "perceived 65% of the time." If Weber's law actually holds in any special realm of sensation it ought to be expressed without the ambiguous term "just perceived."

The examination of a whole series of differences has brought out the fact that no particular one in the series possesses any intrinsic superiority as a starting point. That very fact points the way to a more far-reaching statement of Weber's law; for if one full series presents a uniform increase in the number of correct judgments as the differences increase, it is a fair hypothesis to suppose that the same relative differences on another standard will bring out a similar uniform series. If, then, the two tables of frequencies for the same relative differences show a substantial agreement within any limits of variation that may be set, it will be true that Weber's law holds, not only for the "threshold," but for any difference whatever. The general statement of the

[22] Ebbinghaus, *Grundzüge*, 1905, I, p. 521; James, *Briefer Course*, p. 18 ff; Myers, *Experimental Psychology*, 1909, pp. 255-56.

law will then be, *The same relative difference will yield the same proportion of correct judgments with stimuli of different magnitudes.*

A subsidiary series (Groups B, D, F, Table II, and middle curves, fig. 1), carried out with a standard weight of 50 grams under exactly the same conditions as those of Groups A, C, E with the 100-gram standard, shows not only that the distribution of judgments is as regular in the one case as in the other but that the frequencies are only slightly different for the same relative differences from the two standards. The choice of 50 grams here (determined by a desire to avoid fatiguing the subject) was not particularly favorable for a demonstration of Weber's law, nor is the method of operation (full arm lift) the best, so that the amount of agreement found is surprising.[23]

It is by no means necessary, however, that experiments intended principally to bear upon Weber's law should be carried to such great lengths as in this case. Groups G to O of Table II show the force of the evidence from a full series even when the number of cases is comparatively small. There is substantial agreement between the series, but in the long run an increase in absolute weight brings an increase in the number of correct judgments for the same relative amount of difference. The effect of a change of 50 grams in the standard may be as great as that produced by an increase of 2% in the difference; thus (Groups M, N, O, Table II) 7% on 150 grams gives more right answers than 9% on 100 grams or 11% on 50 grams. This influence is stronger when the second weight is greater (A, B) rather than less (C, D) and when it lies to the left (G, H, I) rather than the right (J, K, L). The divergence between the series does not seem to depend upon the amount of the difference, the two curves of distribution running almost parallel (middle part of fig. 1).

In this case the bare array of figures is sufficient without any

[23] This method of testing Weber's law is an extension of Fechner's. He used the same two proportional differences on all the standards instead of trying to get a difference which would give the same proportion of right judgments. The full series gives either the relative number of judgments on the same difference or the differences which give the same number of right judgments.

calculation to show that, as was to be expected, for lifted weights as light as these there is a considerable departure from Weber's law.

As a rule, such a set of full series with different standards, even if the number in each series were small, would be more serviceable than the usual large accumulation of cases on only a few differences. If this method can be adopted (and there are many cases in which its application is simple) the fact that the degrees of difference on the one hand and the differences in intensity of the stimuli on the other hand form closely arrayed series in two directions will serve as a complete check over irregularities depending on the small number of cases. It should always be remembered, however, that the ideal experiment would involve a sufficient number of cases so that each result in the intensity and difference series could be depended upon by itself without reference to its compatibility with its neighbors in either series. The number of cases required depends entirely on the regularity of the results measured by fractioning them into smaller groups.

The series with minimal differences (Table III) gives a number of right answers for the difference of + 0.4 and smaller differences very different from the numbers found in Table II, and the series based on judgments of "same" or "heavy" (Table IV) gives still other figures. In the first case there can be little doubt that the change is due to the entirely different range of differences used. When only very small differences are used the number of right answers is increased enormously toward the upper end of the range. In the other case the effect comes as a result of changing the terms in which judgment is expressed. In any event it is clear that a valid test for Weber's law must be based upon series which involve the same number of terms and which cover as nearly as may be the same range of proportional differences, while the terms of judgment must of course be the same. The number of right answers is not simply a function of the standard and the amount of difference. It depends very largely upon other considerations which are not

always obvious, and not the least of these is the concatenation of other differences which are judged before and after the difference in question.

It is for this reason, if for no other, desirable to have the whole series of differences worked over together. The insertion of new members in the course of the series or the subtraction of those which seem superfluous cannot fail to result in a change of attitude toward those which remain. When the full series of differences from one standard is compared with an exactly similar series for another standard, there is bound to be a certain amount of time lost and a busy experimenter is tempted to reduce the number of trials in the series. This tendency is given full rein in the "method of least noticeable differences" by the adoption of some arbitrary rule to determine the upper limit of the range of differences. With many experimenters it is customary to stop the series when three successive right answers have been given, no matter what the actual amount of difference reached, and as a consequence no two series are of the same length.

On the other hand, the usual form of the "method of right and wrong cases" in which only a few differences are used is too simple. The particular amounts of difference chosen may be subject to some special influence. And if they fail to yield for different standards nearly the same number of right cases the result is not equal to a demonstration that Weber's law does not hold. But as more and more differences are taken, until in the ideal case the full series is covered, the evidence becomes more and more trustworthy. The very complexity of the full series gives protection against the particular influences that affect single differences. The determination of any single point on the curve of distribution can be corrected by the determination of its neighbors.

But the essential point in regard to Weber's law is not whether a certain proportional difference is "just perceptible" with different standards or even whether it is equally perceptible in terms of the proportion of right and wrong answers, but rather

whether the distribution of judgments of any particular type is of the same form for different standards. This is a question which can only be answered by experiment, and that means an experimental trial of the whole series for each of the standards under consideration.

X.

PRACTICE.

Work was begun on these experiments with only a brief "warming up" on the first day. In order to keep the attention up to as constant a pitch as possible, the observer was informed of each error throughout the first 22 series of experiments, Tables

TABLE VIII—PRACTICE.

The average number of correct judgments for each series, grouping several amounts of difference together.

Series	Standard in Grams	2nd Weight in Relation to 1st	Differences 1–6	7–12	13–18	1–18
1	100	left	69.8	88.2	96.5	84.8
2	100	left	72.5	89.6	97	86.4
3	100	left	68.3	88.1	96.9	84.4
4	100	left	67.8	90.2	97.2	85.1
5	100	left	67	87.5	95.5	83.3
6	50	left	68	87.1	93.8	83
7	50	left	64.1	84.5	94.7	81.1
8	50	left	67.6	88.3	97.4	84.4
9	50	left	65.5	88.3	98	83.9
10	50	left	63	91.5	97.2	83.9
11	50	right	64.1	85.3	97.2	82.2
12	100	right	73.8	93.5	99.2	88.8
13	100	right	72.9	94.2	99.5	88.9
14	100	right	68.6	91	99.2	86.3
15	100	right	67.1	92.5	99.4	86.3
16	100	right	69	90.7	98.5	86.1
17	100	left	68.8	92.6	97.7	86.4
18	100	right	70.5	91.8	98.3	86.9
19	100	left	71.1	95.7	99.8	88.8
20	100	right	72	92	99.2	87.7
21	150	left	75.2	95.2	99.3	89.9
22	150	right	71.1	92.3	98.2	87.2
Average			*68.9*	*90.5*	*98.1*	*85.7*
23	100	left	73.1	94.3	99.7	89

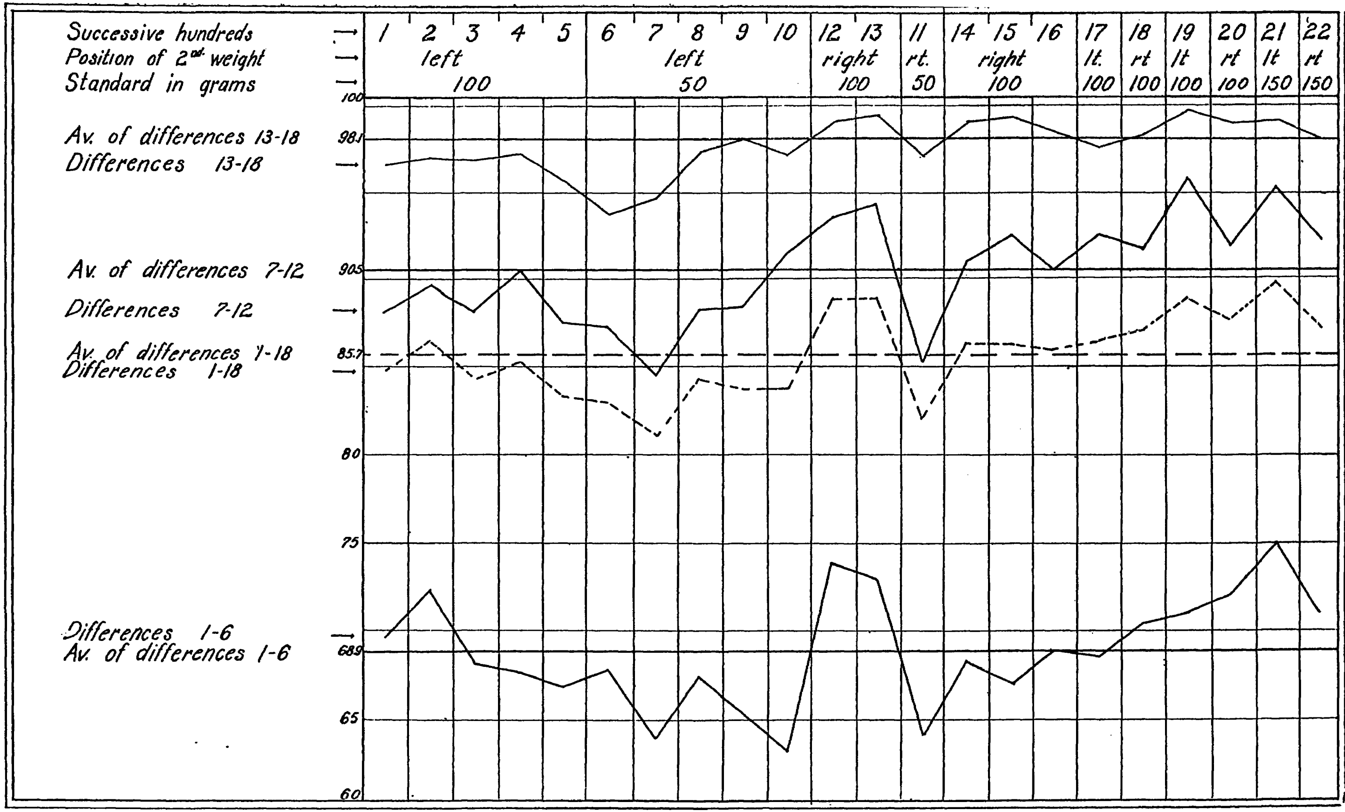


Fig. 4.—The per cent. of correct answers (measured from 60 up) for each 100 trials (counting from the left) when several amounts of difference are counted together. Also the average for each such group for 2200 trials. Data from Table VIII.

I and II,[24] but not of the particular amount of difference involved, so that there was no learning of particular pairs. It would be natural, therefore, to expect some improvement with practice, but none occurs during the first five series, each of which embraces 100 comparisons of each of the nineteen differences. The columns of Table VIII give the average number of correct judgments over the whole range of differences from 1% to 18% and also over a range of six differences making the lower, middle, and upper thirds of the whole range. The same results appear also in fig. 4 with the exception that the "11th" hundred is restored to its proper chronological order, being displaced in the tables for the sake of clarity.

After the first six series there is irregular improvement in the judgment of the larger differences (7-18) but none for the smaller differences (1-6). The whole range (1-18) shows slight improvement. This practice effect appearing only after ten thousand trials does not admit of easy explanation along familiar lines. It can hardly be attributed to the corrections received.

The improvement shown is in the way of greater constancy as indicated by the lopping off of the coarser errors, and such a decrease in variability without any noticeable increase in sensitivity may be attributed, perhaps, to long habituation and the adoption by the observer of a more automatic attitude. It is possible also that this is an instance of the effect of the absolute weight,[25] the larger weights coming in time to stand out with greater distinctness from the general level of the average as the latter grows increasingly familiar. The improvement shown is not, however, as regular as it might be expected to be if this tendency were constantly operative.

Apart from the general practice effect and without reference to the space order, there are whole series of a hundred comparisons (like the 12th, 13th, 19th) in which unusually keen

[24] Müller, *Gesichtspunkte,* p. 24, states the advantages and dangers of correcting the judgments. It should be added that such corrections are justifiable only when there is a considerable number of mistakes; otherwise the momentary sharpening of attention just after each correction will become a source of error.

[25] Martin and Müller, p. 44.

attention is indicated by a decrease in the whole number of errors large and small. The experiments of such a series occupied five or six days. A period of inattentiveness occurred at the seventh hundred, and throughout the first group of five series there was a gradual falling off of attention in this sense. A similar gradual depression affected in less degree the group from the twelfth to the sixteenth hundred.

The "11th" hundred was introduced after the "13th" with the expectation of restoring the attention, which had begun to lag at the end at the end of the thirteenth, to the level of the twelfth on the supposition that that high level was attributable to the lighter weight of the preceding five series. This expectation was not justified and this high mark was attained in only one (the nineteenth) of the remaining series with this standard. After the completion of all the other work with this same standard but under various conditions, another series (23 in Tables I and VIII) was taken under the same conditions as those of the first five series. The result again, as in the twelfth and thirteenth series, is a decided improvement, which is particularly noticeable in the decreasing number of coarse errors. The previous case renders it doubtful whether this high level could have been maintained for long. It may be due to increased power of discrimination acquired in the course of some 73,000 trials, or it may simply be the result of better attention evoked by the great number of very small differences in the intervening series.

Instead of a well-defined practice effect no more appears than a slow oscillation in the number of errors together with an irregular tendency toward the elimination of unusually large errors. Such sluggish but steady changes of attitude on the observer's part are further evidence of the primacy of those mental postures which we call states of attention both in determining the limits within which errors occur and in controlling the frequency of errors of any particular magnitude. It is probable that the general improvement with practice springs from the same source as the monthly or weekly ups and downs.

No practice effect appears in the series with minimal differ-

ences. That is, there is no improvement in the ability to discriminate the weights correctly. There is, however, as the totals at the right of Table III show, a constant development of the tendency to call the second weight heavier. The superficial appearance of this tendency is the same as that of a genuine case of improvement with practice, but its real effect is entirely different. The change of attitude which leads to an increase in the number of judgments "second greater" is not in the direction of better adaptation. Instead of increasing the actual ability of the observer to judge the weight correctly and so to cope better with a concrete situation, it really works against the observer's interests. It increases an already large constant error and produces a still further dislocation of the point of apparent equality; the increase in ability to distinguish 100.4 from 100 is got at the expense of a loss in ability to distinguish 98 from 100, and the net result is that the individual is less able than before to give a judgment that is objectively right in case he is called upon to distinguish two weights in practical life.

This change of attitude takes place without any interference from the experimenter. No corrections were given and the observer had no occasion to think that some answers rather than others were right. At the end of the set of 14,000 comparisons the observer did not believe there were more judgments "left greater" than "right greater," although there were really 1572 more, or about 13 of one type to each 10 of the other type.

In a sense, the development of a bias in favor of one alternative rather than the other may be attributed to practice. It is undoubtedly one mode of adapting one's self to a situation which is somewhat distasteful. The observer is obliged to say either one thing or the other, there is no reward or penalty attached to either one or the other, and he finds it easier to say this rather than that. The original preference may have an obscure origin or may result from mere chance, but once having gained a foothold it continually increases in force.

Such a preference may be stated in terms of mental life as an unreasoned prejudice or a dread of saying one thing too often

and a consequent tendency to say the other thing too often. It may arise easily from accidental circumstances which act as suggestive hints to the mind hard pressed for some basis on which to form a judgment in the absence of a clear sense-difference. In the language of physiological psychology, such an increasing tendency indicates the formation of a neural habit, with an ever-increasing facility of discharge along one path of expression rather than the other. Practice, instead of increasing the individual's ability to judge, results in further facilitation of this mode of discharge with a corresponding tendency to eschew the disfavored alternative.

It is not probable that there is any sense-difference at the root of this tendency to prefer one mode of expression to another. With minimal differences practice does not result in an increase in the power of sensory discrimination. Only an increase in the constant error occurs, and this is attributable, not so much to an apparent increase in the amount of difference between two given weights, as to an increasing tendency on the observer's part to settle doubtful cases by indicating the second weight.

XI.

VARIABILITY.

The confidence which can be placed in the results of any group of experiments is indicated to a certain extent by the amount of variation between the different series going to make up the group. If successive series of 100 judgments give very discordant results, those results are of less statistical use than the results of more homogeneous series.

TABLE IX—VARIABILITY.

The median and average amount of the mean variation in the several groups.

Group	Table	Relative Position of the Weights	No. of Series in Group	No. of Cases in Series	Median of the Variations	Average of the Variations
P	II.	Left first; standard first	7	50	1.6	1.6
Q	II.	Left first; standard second	7	50	2.5	2.4
R	II.	Left first; standard 1st and 2d	7	100	2.3	2.6
S	II.	Right first; standard first	7	50	1.5	1.8
T	II.	Right first; standard second	7	50	2	1.7
U	II.	Right first; standard 1st and 2nd	7	100	2.2	2.5
V	II.	All orders	14	100	2.7	2.8
Minimal	III.	Left first; standard first	10	100	3.7	4
Sameness	IV.	Left first; standard first	6*	100	5.6	4.9

* The first six differences only; from —0.5 to +3.

Table IX shows the average and median value of the mean variations in different divisions of the experiment. Neither way of expressing the value is quite satisfactory, and it will be necessary to refer to the original tables for more explicit data. Of the groups in Table II, those which afford the greatest number of correct answers have regularly the lowest variations. The arrangement with the standard first gives very much more constant results than that with the standard second.

Group P (second greater, left) has the lowest mean variations as well as the greatest number of correct judgments, and Q

(second less, left) is at the other extreme. On the whole, both time orders together, the arrangement with the second weight to the right gives the most reliable results, particularly for the larger differences, as well as the largest number of correct judgments. It should be remembered, however, that this group (U) was begun after the completion of the larger part of the other group (R). In Group R there was a constant tendency to prefer as heavier the first (right-hand) weight. The observer became conscious of this tendency and, though its existence was denied by the experimenter, made ineffectual efforts to overcome it. This fact might account for the greater irregularity of the whole group but not for the great advantage of one time order (P) over the other (Q). This distracting idea did not occur when the second weight was on the right-hand side, and in that group (U) there is less difference between the two time orders. The observer felt that the absence of a constant tendency (so far as she knew) made the judgments of Group U easier. This feeling, which arose in spite of the fact that the arrangement of Group R (second, left) was chosen at the beginning as more natural, may be connected as cause or as effect with the better results obtained; that is, the feeling may indicate that the conditions were actually better, or it may arise simply from the slight decrease in the number of corrections received.

Since there are constant tendencies for each time and space order, it is evident that the mean variation of single groups should be less, as a rule, than that of combined (time—or space—order) groups. This result is most distinct when the constant tendencies are most marked. In general, however, the average for the combined group (V), being based on double the number of cases, has a lower probable error than the averages of the simple groups. When it is not certain which simple group will have the best conditions for attention or judgment, the safest inferences, with a minimum of experimentation, can be drawn from the combined group.

The mean variations for the series with minimal differences are larger, on the whole, than those of the main series just men-

tioned. Although it is not possible in this case to observe any decrease in the variability as the differences become larger, yet the series as a whole confirms the data of Table II in that the variability is found to be larger when the conditions of judgment are such as to permit of only a small majority of correct answers.

With regard to the choice of an arbitrary "threshold" for the comparison of individuals or for testing Weber's law, it is a pertinent fact that the general increase of right judgments due to increasing the amount of difference between the weights is followed by a decrease in the mean variation. A few observations on a large difference are therefore as significant as a larger number on a smaller difference because of the lower probable error of the average when the difference is large. There is nothing to indicate that a "just noticeable difference," *e.g.*, one giving 75% of right answers, gives particularly reliable results from this point of view; there is positive evidence that a larger amount of difference affords a truer measure of the observer's average ability.

If mistakes in judging difference are of the nature of lapses of attention, they may be attributed to distraction. There are always a certain number of conditions present which tend to reduce the maximum of mental efficiency and it may be presumed that these amount to a constant distraction. With lessening amounts of difference such constant distractions nullify more and more frequently the influences of the difference. A change in the regular conditions, such as the substitution of a less favorable space or time order, is equivalent to the introduction of a fresh distraction, and the result is an increase in the number of mistakes.

Constancy of attention may be inferred from a relatively low variation in the number of errors for any given amount of difference or for a whole group. A low variation may also mean the elimination of excessively large errors as has been claimed above. At any rate, the experiment shows that a low mean variation accompanies a decrease in the total number of errors whatever the cause of that decrease. Conditions of keenest dis-

crimination are also conditions of most reliable judgment; states of steady attention are associated with states of sharpened attention. The frequency of lapses of attention can be predicted with greatest assurance when the whole number of lapses per hundred is least. This minimum of errors is attained under conditions of easiest attention, that is, when a large difference and a positive constant tendency unite to increase the number of right judgments. On the other hand unfavorable conditions, while reducing the number of right judgments, reduce at the same time their reliability.

To free the observer from strain upon his attention is desirable for itself and, inasmuch as so doing increases the statistical value of his judgments, there can be little question that large differences rather than "threshold" values are the best substitute for a complete series of differences in cases when the complete series is precluded.

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 2, pp. 73-197, 6 text-figures November 12, 1910

THE PROCESS OF ABSTRACTION

AN EXPERIMENTAL STUDY

BY

THOMAS VERNER MOORE.

CONTENTS.

INTRODUCTION.

The decade that is just now drawing to a close has witnessed a notable extension of the field of psychological research. In the beginnings of modern psychology the field of experiment was seldom extended beyond the domain of sensation. Progress in physics and physiology made it possible to subject our sensations to experiment, and for some time the sensory processes received the chief share of the attention of psychologists. It was not long, however, before the emotions began to receive their due amount of consideration and the invention of the plethysmograph opened the way to a new line of research. But only within the last ten years has the experimental study of such higher processes of thought, as abstraction, commenced to develop. The impetus to this new development has come mainly from Professor Oswald Külpe at the University of Würzburg. The present research, although its origins are not to be traced to the school of Würzburg, belongs to the field which Professor Külpe and his students have so admirably developed. Our problem was to study the mental processes involved in the formation of our abstract ideas. It is indeed true that the very existence of such ideas has been called in question. Still we may at present assume, for the purpose of stating our problem, that it is possible for the mind to perceive a series of objects, and to recognize some one quality or group of qualities as recurring constantly in every member of the series. The botanist examining a set of specimens will classify them according to certain characteristics which mark off the genera and species. The group of characteristics constitutes what may be termed his concept of the genus or species that he has segregated. Of each species he has a more or less definite "concept," by which he can represent to himself a number of specimens, no two of which are precisely the same. Such "concepts," whatever may be their real nature, are facts of conscious experience; we form

them and use them incessantly. But what after all is the "concept"? What is the process of its formation? This is the problem of the present research.

The history of the problem dates back to the days of the Greek philosophy; but only within the last few years has it been subjected to an experimental investigation. The more recent literature is of immediate interest for our present problem. The metaphysical discussions, valuable as they are within their own sphere, bear only indirectly on the experimental question. Consequently, only the experimental literature bearing in some manner on the process of abstraction has been analyzed. Not every allusion in the extensive psychological literature of the day could be picked out, but a general account of the important pieces of experimental work from Galton to the present time has been given. The individual studies have been analyzed with some completeness because the history of the literature is an integral part of the evidence on one point of the present study, *viz.:* Is there or is there not a distinction between thought and imagery; and if so, in what sense is thought to be interpreted?

I.

LITERATURE OF THE PROBLEM.

The first experiments which in any way approached the domain of our abstract ideas were made by Francis Galton in 1878. In the *Proceedings of the Royal Institution of Great Britain* for 1879[1] was published his memoir on Generic Images. In this article he refers to an earlier one in the *Journal of the Anthropological Institute* for 1878.[2] The bearing of these experiments on abstraction is suggested rather than direct. But they have become the basis of the now famous composite-image theory of ideas, and are therefore deserving of mention. Galton described in this article the composite photographs which he had just succeeded in obtaining. These he compared to "our general impressions." Just what he meant by "our general impressions" is not clear; but he congratulates himself that his explanation coincides with that of Professor Huxley in his work on Hume. "I am rejoiced," he says, "to find that from a strictly physiological side this explanation is considered to be the true one by so high an authority, and that he has, quite independently of myself, adopted a view which I also entertained, and had hinted at in my first description of composite portraiture, though there was no occasion at that time to write more explicitly about it."[3]

Huxley's meaning is clearer, and to him we may turn for an outline of the theory. In the above-mentioned work on Hume, the following quotation gives a clear idea of the generic image theory of general concepts.

---

1 Pp. 161-171.

2 This article is mainly concerned with the technique of composite photographs.

3 *Proceedings of the Royal Institution of Great Britain,* 9, 1879-1881, p. 166.

"Now when several complex impressions which are more or less different from one another—let us say that out of ten impressions in each, six are the same in all, and four are different from all the rest—are successively presented to the mind, it is easy to see what must be the nature of the result. The repetition of the six similar impressions will strengthen the six corresponding elements of the complex idea, which will therefore acquire greater vividness; while the four differing impressions of each will not only acquire no greater strength than they had at first, but in accordance with the law of association, they will appear at once, and will thus neutralize one another.

"This mental operation may be rendered comprehensible by considering what takes place in the formation of compound photographs—where the images of the faces of six sitters, for example, are each received on the same photographic plate for a sixth of the time requisite to take one portrait. The final result is that all those points in which the six faces agree are brought out strongly, while all those in which they differ are left vague; and thus what may be termed a generic portrait of the six, in contradiction to a specific portrait of any one, is produced. . . .

"The generic ideas which are formed from several similar, but not identical, complex experiences are what are commonly called *abstract* or *general* ideas; and Berkeley endeavored to prove that all general ideas are nothing but particular ideas annexed to a certain term which gives them a more extensive signification, and makes them recall, upon occasion, other individuals which are similar to them. Hume says that he regards this as 'one of the greatest and the most valuable discoveries that has been made of late years in the republic of letters,' and endeavors to confirm it in such a manner that it shall be 'put beyond all doubt and controversy.'

"I may venture to express a doubt whether he has succeeded in his object; but the subject is an abstruse one; and I must content myself with the remark, that though Berkeley's view

appears to be largely applicable to such general ideas as are formed after language has been acquired, and to all the more abstract sort of conceptions, yet that general ideas of sensible objects may nevertheless be produced in the way indicated, and may exist independently of language.''[4]

It would thus seem that Huxley's theory—and probably Galton's also—is that only our abstract ideas of *sensible* objects are to be compared with the composite photographs. Galton points out that the mind in forming its generic images is much less perfect in its mechanism than the camera. ''Our mental generic composites are rarely defined; they have that blur in excess which photographic composites have in a small degree and their background is crowded with faint and incongruous imagery. The exceptional effects are not overmastered, as they are in the photographic composites, by the large bulk of ordinary effects.''[5]

The experiments on composite photographs were not experiments on abstract ideas—they merely suggested a theory of general concepts. Nor did Galton's later experiments on mental imagery[6] approach very much nearer the problem. They called general attention to mental imagery and perhaps stimulated the next investigation of any importance[7] which was made by Ribot.

In October, 1891, M. Ribot published in the *Revue Philosophique*[8] his ''Enquête sur les idées générales.'' This he afterwards amplified in the fourth chapter of his book, *L'Evolution des idées générales.*[9] His problem was this: At the moment of thinking or reading or hearing a general term, what is there in consciousness—immediately and without reflection? On the basis of the imagery which his subjects reported he classified

4 Huxley, *David Hume.* New York, 1879, pp. 92-94.

5 *Loc. cit.*, p. 169.

6 *Cf. Inquiries into Human Faculty*, 1883, Section on Mental Imagery.

7 The article entitled ''Observations on General Terms,'' by S. E. Wiltse, in the *American Journal of Psychology,* 3, 1890, pp. 144-148, was only tentative and contained no definite results.

8 Vol. 32, pp. 376-388.

9 Paris, 1897.

them into: (1) The concrete type (visual or muscular imagery of an object). (2) Typographic visual type (visual image of the printed word). (3) Auditory type. A great many of his subjects said that they had nothing in mind. For example, fifty per cent. of the answers to the imagery of the word '*cause*' said that the subjects had represented to themselves nothing at all. M. Ribot then asked himself the question, What is this "nothing"? The word alone? No. Otherwise there would be no difference between a general term and a word of a language that one did not understand. The word is a sign of some object. We have learned the mental habit of designating many objects that have some point of agreement by this symbol. The objects designated lie hidden and are unconsciously represented by this general term. "General ideas are habits in the intellectual order." Our higher concepts consist of two elements—one clear and conscious, and this is always the word which may at times be accompanied by some shred of imagery. The other element is obscure and unconscious. M. Ribot refrains from saying precisely what this obscure and unconscious element is. From the context it would seem that he means the unconscious trace left by the habitual use of the word to designate various objects.

A word of criticism may be said in passing. M. Ribot's interpretation of this "nothing," which accompanies the perception of a general term, is purely theoretical and is not based on any published data given by his subjects. Furthermore, he has not followed out his sign theory to its logical consequences. For every sign, we have on the one hand the object signified and on the other, the signification. Smoke has on the one hand fire, of which it is a sign, and on the other a signification in the mind of the observer. If, then, general terms are signs, they have on the one hand the objects that they signify, and on the other a signification. This signification, as M. Ribot admits, is not the image and not the word itself. It certainly is not the unconscious factor he speaks of—for he would scarcely maintain that his subjects were not conscious of the meaning of the word. It is therefore a clearly conscious mental process distinct from both the image and the word.

M. Ribot's work comes closer to being an experimental study of the problem than Galton's experiments on composite photographs. Had he examined the state of mind of his subjects when they lacked imagery and not trusted to theory on that point, he would have carried his investigation into the heart of the problem of abstract ideas.

After Galton and Ribot the study of mental imagery took up no small portion of the time and labors of psychologists. But the extensive literature on imagery is not directly concerned with the problem of abstraction.

An excellent piece of work in this field of research is that of William Chandler Bagley of Cornell University.[10] He undertook to study the effect of imperfectly formed words on the perception of spoken sentences, and parts of sentences. The words and sentences were first recorded by a phonograph, the initial, middle, or final consonants being unpronounced. The subject listened to the phonograph, and was called upon to repeat what he heard, and analyze the mental processes he experienced. The section of the work which bears upon our problem is that entitled "The Conscious Process Involved in the Apperception of Spoken Symbols." In comparison with the later German experiments, Dr. Bagley's are remarkable for the very frequent occurrence of imagery of one kind or another. His subjects in perceiving the meaning of sentences report with surprising frequence the presence of visual, auditory or kinesthetic imagery. This might be due to the fact that Dr. Bagley's subjects were capable of sharper introspection than the German psychologists. Still this can hardly be the case. The German psychologists, among whom are such men as Professor Külpe, cannot be supposed to be lacking in the power of introspection. Another possible explanation is that Bagley laid special stress upon the report about imagery and in that way developed in his subjects a "task" to associate definite images with the given sentences.

---

10 "The Apperception of the Spoken Sentence: a Study in the Psychology of Language." *American Journal of Psychology,* 12, 1900-01, pp. 80-134.

Though his subjects generally experienced imagery in the perception of the meaning of a sentence, still he finds cases in which this imagery is lacking. One factor which has to do with the lack of imagery is that "familiarity with the sentence sometimes militates against a clear and direct reference on the part of the observer." In this his results agree with those of Dr. Taylor reported below.[11] Bagley's general conclusion is that,

"The consciousness concomitant with the apperception of auditory symbols is made up of sensational and affective elements—some peripherally, some centrally aroused—in connections which vary in character with different individuals and under different conditions. These connections are arranged in patterns which change rapidly into one another, and are in general transitory and fleeting. When the attention is directed to the peripherally excited elements exclusively—when the external stimuli occupy the burning point of apperception—the meaning which they as symbols should convey is not clearly apperceived. When the attention is directed upon the centrally aroused ideas which the symbols suggest, the 'meaning' is apperceived, but errors and lapses in the stimuli are apt to pass unnoticed." (p. 125.)

He thinks that Stout goes too far in suggesting the existence of representative mental contents different from "visual, auditory, tactual, and other experiences." He thinks that his experiments lead him to no such conclusion. "From the series of observations which were made in the course of our experiments, no conscious 'stuff' was found which could not be classed as sensation or affection, when reduced to its ultimates by a rigid analysis. Neither do our experiments show that there is in the apperception of spoken sentences such a thing as 'imageless apprehension.'" (p. 126.)

Still Dr. Bagley finds something which he does not feel justified in putting down as either imagery or feeling. To this something which is not imagery or feeling and still has to

---

11 See p. 87.

do with the understanding of the sentence, he applies the name 'mood.' "We may say with Stout that the new is referred to a mental 'system,' in so far as such a system is a mood, an attitude, a tendency, an adaptation. The mind adjusts itself uniformly to uniform conditions: this seems to be the essence of the apperceptive 'mood.' When C, in the sentence "The play was bad," interpreted 'play' as a drama, her mind adapted itself in a degree to the drama environment. This was not necessarily a focal reference to a given play, but the mind was in the dramatic 'mood.' Should particular parts of a typical play-environment have been ideally reproduced, the situation would only have been reinforced. Should certain verbal ideas such as 'drama,' 'theaters,' 'Shakespeare,' etc., have been reproduced in consciousness, either visually, auditorily or kinesthetically, these ideas would have been constituents of the dramatic 'mood,' but not necessarily the fundamental constituents. The fundamental constituents may and do vary from time to time. Only very seldom can they be called constant, and the 'constant supplements' which we have noticed are instances of such occasions. The fact that the focal constituents of the apperceptive consciousness are not necessarily consistent with the situation represented bears testimony to this point of view. "There was not room for a stove in the corner"; with this sentence one observer imaged distinctly a stove in the corner of a small, otherwise bare room. His own surprise at the inconsistency of this imagery was shown by his exclamation upon recording the introspection: "But there *was* a stove there!" (p. 127.)

A 'mood,' therefore, is something that has to do with the past experience of the subject in regard to the words of the sentence that is understood. Just what it is, as a present psychical state, Dr. Bagley does not say. It is the revival of past experience. It is not mental imagery, although mental imagery enters in as a partial element in the complex termed 'mood.' If this is so, what is that present psychical state in the mood, which is neither imagery nor feeling? It is not past experience, for the past is not present. It is not revived imagery and

feeling, for Dr. Bagley admits that the mood contains something besides imagery and feeling. It therefore seems that Dr. Bagley has found something more than he is willing fully to recognize. His experiments, like those of the later German writers, reveal the existence of an imageless mental content. Just what we call this is not of prime importance. But its existence should be recognized.

In 1901 appeared Marbe's[12] experimental study of judgment. His main problem is not that of the present work, but it is an early attempt to apply the experimental method to suprasensuous mental processes. He also mentions, as a side issue, his attempt to develop the experiments of Ribot. The work was done in Professor Külpe's laboratory at the University of Würzburg, where Dr. Marbe was at the time Privatdozent of Philosophy. The method is essentially the same as that of the later Würzburg experiments which are described below. His final conclusion in regard to the judgment is that any special mental process—a word or gesture or image—may become a judgment. Taken literally, this conclusion is not borne out by the experiments. What they seem to prove is that a judgment may be signified by a variety of different processes. And this may be the author's meaning (cf. Chapter III). The perception of a judgment, however, is not a sensation or an image or a feeling, or anything that can be pointed out in consciousness. The perception of a judgment is a knowing—a "Wissen" (cf. p. 17).

In his concluding remarks on experimentation in the domain of logic Dr. Marbe mentions Ribot's work, *L'Evolution dés idées générales*, and refers to some similar experiments of his own on ideas and imagery. His meaning is not clear to me, so I quote entire the brief account of his work in this field (pp. 99-101).

"Seit den Zeiten des Sokrates hat man angenommen, daß den Begriffen im Bewusstsein ausser den zugehörigen Worten irgend etwas direkt entspreche, d. h., daß es neben diesen Worten

[12] K. Marbe, *Experimentell-psychologische Untersuchungen über das Urteil.* Leipzig, 1901.

psychische Gebilde gäbe, welche der Gesamtheit der Gegenstände, auf welche sich die Worte beziehen, korrespondieren sollen. Diese Gebilde wurden ursprünglich, wie gelegentlich noch heute im Gegensatz zu den Worten, die nur Zeichen ihrer Bedeutungen sind, als Abbilder derselben aufgefasst, indem sie die gemeinschaftlichen Merkmale ihrer Gegenstände im Bilde enthalten sollten. Solche psychischen Gebilde hat man später je nach dem Grade der Abstraktheit, den man ihnen zuschrieb, bald als Gemeinbilder, bald als allgemeine Vorstellungen, bald als Begriffsvorstellungen bezeichnet. Obgleich, wie bekannt, ihre Existenz schon im Altertum und Mittelalter bestritten und in der Neuzeit hauptsächlich durch Berkeley bekämpft wurde, so hält man doch auch heute noch vielfach in der einen oder anderen Form an derselben fest. Auch die Frage, ob es solche psychologische Äquivalente der Begriffe giebt, ist eine rein psychologische, und ihre Behandlung sollte nicht, wie es in der Regel geschieht, mit logischen Untersuchungen vermischt werden. Die Aussagen unserer Versuchspersonen über die Bewusstseinsvorgänge, welche sie nach dem Erleben von Urteilsworten und Urteilssätzen zu Protokoll gaben, enthalten übrigens nichts von solchen Parallelerscheinungen der Begriffe, ebensowenig, wie die wertvollen Untersuchungen von Ribot,[13] in welchen dieser Forscher einer Reihe von Beobachtern Substantiva zurief, um sich dann von ihnen sagen zu lassen, was die gehörten Worte für Erlebnisse auslösten. Ich selbst habe mehreren Beobachtern ca. 20 Substantiva zugerufen und mir dann von ihnen berichten lassen, was für Erlebnisse die zugerufenen Worte erzeugten. Dann gab ich denselben Beobachtern der Reihe nach verschiedene Karten in die Hand, auf welchen jeweils ein Substantivum aufgedruckt war, während sie nach einigen Augenblicken die Erlebnisse zu Protokoll geben mussten, die durch den Anblick der gedruckten Worte in ihnen ausgelöst wurden. Endlich stellte ich ihnen die Aufgabe, die Begriffe: Baum, Volk, Gesellschaft, Zeit u. a. zu denken und mir dann die Resultate ihrer Bemühungen mitzuteilen. In allen diesen Fällen zeigten die Proto-

[13] *L'Evolution des idées générales.* Paris, 1897, p. 127ff.

kolle nichts von Begriffsvorstellungen u. dergl. Die Erlebnisse der Beobachter bestanden vielmehr ausschliesslich in Wahrnehmungen, Vorstellungen und Bewusstseinslagen, die teilweise gefühlsbetont, teilweise ohne jeden Gefühlston verliefen. Man wird also wohl sagen dürfen, daß es keine psychologischen Äquivalente der Begriffe im Sinne der Begriffsvorstellungen giebt. Jedenfalls aber sehen wir leicht ein, daß auch diese Frage experimentell behandelt werden kann und muß und daß sie die Logik weiter nicht tangiert."

The obscurity arises from the fact that it is not perfectly clear whether Marbe merely denies the existence of the general images which Bishop Berkeley[14] termed abstract ideas, or that he claims that there are neither general images nor universal concepts.

It would seem, however, that Marbe found no evidence for the existence of a general image in the understanding of the words given to his subjects. He did find, however, *Wahrnehmungen, Vorstellungen,* and *Bewusstseinslagen.* This latter is a term introduced by Mayer and Orth[15] to represent certain "states of mind" which are more or less refractory toward all

[14] "Whether others have this wonderful faculty of abstracting their ideas, they can best tell; for myself I dare be confident I have it not. I find indeed I have indeed a faculty of imagining, or representing to myself, the ideas of those particular things I have perceived, and of variously compounding and dividing them. I can imagine a man with two heads, or the upper parts of a man joined to the body of a horse. I can consider the hand, the eye, the nose, each by itself or separated from the rest of the body. But then whatever hand or eye I imagine, it must have some particular shape and colour. Likewise, the idea of man that I frame to myself must be either of a white, or a black, or a tawny, a straight or a crooked, a tall or a low, or a middle-sized man. I can not by any effort of thought conceive the abstract idea described [in his previous account of the abstract ideas of the traditional logic]. And it is equally impossible for me to form the abstract idea of motion distinct from the moving, and which is neither swift nor slow, curvilinear nor rectilinear; and the like may be said of all other abstract general ideas whatsoever." *A Treatise concerning the Principles of Human Knowledge.* Introduction, 10, pp. 141-142, vol. I of Fraser's Oxford (1871) Edition of his Works.

It is evident from the context that Bishop Berkeley does not distinguish between the mental image and the abstract concept—between what is termed by the later Würzburg School the *Vorstellung* and the *Begriff.*

[15] "Zur qualitativen Untersuchung der Association." *Zeitschrift für Psychologie und Physiologie,* 26, 1901, p. 6.

analysis—and in which images (*Vorstellungen*) are not to be found. In a later study[16] Dr. Orth attempted to show that the *Bewusstseinslage* was not a state of feeling.

That part of Dr. Narziss Ach's work *Über die Willenstätigkeit und das Denken,*[17] which refers to thought has a direct relation to this line of work, which we may consider as originating in Ribot's "Enquête sur les idées générales." The *Bewusstseinslage* of Marbe appears under the name of *Bewusstheit.* This author too has recognized the existence of mental states in which there "could not be detected any such phenomenal elements as visual, auditory, or kinesthetic sensations or memory pictures of such sensations which qualitatively determined the mental content reported as knowledge" (p. 210). There are often present along with such states of consciousness words or fragments of words. "Such a presence of kinesthetic or auditory kinesthetic images may well be the cause of the widely disseminated hypothesis that our thought continually takes place in an inner speech or adequate visual, acoustic, or similar kinds of memory images. Against such a view one must point to the fact that there are very complex contents in which, as already mentioned, the partial contents are consciously represented in their manifold opposing relations and still these individual contents are not expressed by any adequate vocal designations and the like—and indeed, it is absolutely impossible that they should be" (p. 215).

The question then arises, what are these imageless states of consciousness? This Dr. Ach explains by an example: "Every idea which is given in consciousness, for example, the word 'bell' puts, as is well known, a number of ideas in readiness, with which it stands in associative connections. This putting of ideas in readiness, or stimulation of tendencies to reproduction, suffices for the conscious representation of what we call

[16] Dr. Johannes Orth, *Gefühl und Bewusstseinslage.* Sammlung von Abhandlungen aus dem Gebiete der Pädagogischen Psychologie und Physiologie, edited by Ziegler and Ziehen. Vol. 6, No. 4. Berlin, 1903, *cf.* especially pp. 69-75.

[17] Göttingen, 1905.

'meaning' without its being necessary that the ideas should actually become conscious" (p. 217). A nonsense syllable or a word in an unknown language does not place in readiness any such set of tendencies to reproduction and consequently has no meaning. Every signification and every idea is an associative abstraction because it picks out some of a vast number of possible associations. And no signification is identical with any other but only more or less analogous.[18]

The problem of the understanding of words and sentences was taken up by Dr. Clifton O. Taylor in 1906.[19] His first experiment was based upon a similar one made by Marbe in the "Philosophischen Gesellschaft" during the winter semester of 1904, and may be considered as a continuation of Marbe's line of work. He read to his subjects the following sentence: "Imagine that in a rectangular space a plane is laid passing through the upper and lower edges of two opposing sides. The plane then must stretch obliquely through the space. How many such planes can you imagine in this space?" Then followed seven subordinate tasks based upon this fundamental problem.

From the protocol obtained from his subjects it was evident, that for the understanding of sentences expressed in concrete terms the development of mental images can be useful, but that they are not indispensable. These auxiliary images become less frequent the more familiar the subject is with the text.

A second experiment was carried out in which the subject read a text from Gegenbauer's "Anatomie." He had to take care that he understood the text perfectly, and while reading marked the places where he experienced any mental imagery. Visual imagery aided materially in understanding the text. But on writing out the text and then rereading the written copy, the imagery was reduced from fourteen pictures to but one in the third reading. On the other hand, from experiments with

[18] For a criticism of Dr. Ach's view see below, p. 181 ff.

[19] "Ueber das Verstehen von Worten und Sätzen." *Zeitschrift für Psychologie*, 40, 1906, pp. 225-251.

sentences composed of abstract terms, it seemed that the appearance of images hindered rather than helped the understanding of the text. With Taylor, as with other members of this school, the *Bewusstseinslage* comes into prominence, and it is found that these attitudes of consciousness are the *more* frequent as the subject is *less* familiar with his text.

We must now go back a few years to an independent line of research. Two years after the appearance of Marbe's experimental study of judgment, Binet published his brilliant *L'Étude expérimentale de l'intelligence.*[20] His experiments were commenced before the appearance of Marbe's work, for we read on page 76 of a series made in November, 1900. Binet may therefore be considered as a real pioneer in this field. He must too have exercised no little influence on later German authors, for he showed how the method of controlled introspection could give very valuable assistance in the study of our higher mental processes. He used in his experiments a variety of subjects, but most of all his two daughters, Marguerite fourteen years old, and Armande thirteen. His first experiment was to give them the task of writing twenty words, any that they might wish. By questioning he then found out in what sense the words had been used, and how they came to be thought of. By classification of the words used in repeated experiments M. Binet found very characteristic differences in the vocabularies of his two children. Their environment having always been the same, this difference, he concluded, was due to their temperaments. Temperament therefore has its influence on our choice of words. From the fact that the words written formed well-defined groups, M. Binet concluded that association alone does not entirely account for our train of thoughts. Association accounts for word after word in any group, but it does not account for the origin of a new group.

In another series of experiments the two little girls were given a word and instructed to tell their father of what they had thought. Binet was able to analyze this experiment into

[20] Paris, 1903.

the following stages: (1) The hearing of the word. (2) The perception of its sense. (3) An effort to call up an image or determine a thought. (4) The appearance of the image. One of the observations of Armande shows very clearly the distinction between stages two and three. "As yet there are no images (at the moment of choice) and I know why there are none: When there are many things such—for example, a house, there are many houses—it is necessary to choose. Just then I think about it without representing anything to myself as an image" (p. 75). Sometimes, however, says Binet, the image comes without being sought.

Binet gives a special chapter to the problem of thought without images. The conclusion at which he arrives is that neither visual imagery nor internal words, either alone or together, account for that complex mental process which we term thought. The grounds for this conclusion are the many instances in which his subjects had not and could not find any visual imagery for their thoughts. And again there were times when he thought that he could determine that word-imagery was also entirely lacking.

Binet also attempted, and with success, to have his little girls give a rating for the clearness of their images. These ratings ranged from 0 for very weak images up to 20 for images as clear and well defined as actual sensations of sight.

In Marguerite there were three well-defined groups:

I. A group in which the rating was usually 20, or a little below. This group contained memory images of well known objects or things recently seen.

II. A group in which the rating ranged from 10 to 15. This group contained memory images of objects not recently seen.

III. A group in which the rating ranged from 3 to 6. This contained memory images of things read or heard about and fictitious images of imagination.

With his other subject, Armande, the differences were not so clear. M. Binet gave up hope of finding any regularity. He published, however, the ratings for the three classes. The

averages, which he did not give, are for Class I, 6.2; Class II, 4.9; Class III, 2.8. Considering that Armande's ratings ranged from 0-12, and Marguerite's from 0-20, one would not expect the classes to be so well defined. The ratings of Armande, however, show the same tendency as those of Marguerite—only not so marked. The small number of cases, however, leaves the result uncertain.

In his discussion of the theory of abstract thought and images M. Binet says that his data would support any theory. He makes this claim on the basis of a strange assumption that the discussions between nominalists and realists and conceptualists have always concerned images and not thoughts. He does not exactly state this assumption but it is evident from the text. He then comes to his *intentional* theory of the image. The image may be used by the person to represent a particular or general signification. It represents whatever the subject *intends* that it should. He would place, therefore, intentionism as a new theory alongside of realism, nominalism, and conceptualism. With Binet then there is thought, image, and object. The image is an arbitrary sign to which the subject gives at will a particular or general significance. In our mental life there are those distinct classes of phenomena—thought, image, and interior language. Association alone does not account for the mechanism of thought. It is more complex and supposes constantly such operations as choice and direction. The stream of thought is far wider and deeper than that of our imagery. The last sentence of the book is this: "Finally—and this is the main fact, fruitful in consequences for the philosophers—the entire logic of thought escapes our imagery."

The next experimental work on abstraction was that of Professor Külpe. A report of this was read at the German Psychological Congress, which met at Giessen in the summer of 1904. This was the beginning of a series of experimental studies by several of Külpe's students in his laboratory at Würzburg.

The first experiments of Professor Külpe were made in the

summer of 1900, with Professor Bryan of Indiana.[21] Külpe was not satisfied with these and decided to take up the problem again with improved methods. By means of a stereopticon lantern he projected upon a screen in a dark room the figures that were to be observed by the subject. The objects projected were nonsense syllables, four in number, which were grouped at equal distances around a given point of fixation. Each nonsense syllable consisted of a vowel and two consonants. The syllables might be in four different colors—red, green, purple, or black. In the different experiments also the four syllables were grouped so as to form various figures. A group of syllables forming with their different colors some kind of a figure was termed by Külpe an object. The subject could be instructed to observe the object from some definite point of view, or he could be left to observe the object without any prescribed task. There were four points of view given to his subjects:

1. The determination of the entire number of letters visible.

2. The determination of the colors and their approximate positions in the field of consciousness.

3. The determination of the figure which the grouping of the syllables formed.

4. The determination of as many letters as possible, with their positions in the field of vision.

The number of statements possible to any subject about the individual letters could be classified as follows: (*a*) The entire number of statements made; (*b*) the correct statements; (*c*) the incorrect; (*d*) the indeterminate statements; and (*e*) those that could have been made but were not. Each division could then be rated by its proper percentage of the entire number of statements. Where task and statement come together (*i.e.*, in the statements about the task) the percentage of correct (*b*) statements is a maximum and that of unmade (*e*), indeterminate (*d*), and in general also false (*c*) statements is a minimum. This

[21] O. Külpe, "Versuche über Abstraktion." *Bericht über den I. Kongress für experimentelle Psychologie in Giessen, 1904.* Leipzig, 1904.

proves that "Abstraction in the sense of an accentuation of certain portions of a mental content, *i.e.*, positive abstraction, succeeds best when a preoccupation of consciousness—a predisposition for the partial content, is given or provided for." (*op. cit.*, p. 61.)

Negative abstraction—the tendency to neglect or forget all but the one thing abstracted—is the more complete the greater the difficulty of the task.

In explaining these results Külpe asks what is the reason for the effect of the task? "Were," he writes, "the *elements* or the *colors* seen differently under the influence of corresponding or heterogeneous tasks, or were they apprehended (*aufgefasst*) differently? . . . According to our protocol and the entire conditions of the experiment, to that question one can only answer that the difference lies merely, or at least chiefly, in the apprehension and not in the sensations" (p. 66). The task does not affect sensation but it does affect apperception. If that is so, then there must be a distinction between sensations and our perception of them. "That this distinction must be made in much the same sense in which we distinguish between physical phenomena and our consciousness of them; that, in other words, the old doctrine of an inner sense with the involved idea of a distinction between the reality of consciousness and objectivity must now have its opportune renewal in the domain of psychology—this is the principal result that I would draw from my experiments." (p. 67.)

Henry J. Watt, a student of Professor Külpe, published[22] in 1905 his ingenious attempt to approach the experimental treatment of the supra-sensuous mental processes by a study of reactions of association. As is well known, the reaction-time of association was originally measured by experiments in which the subject was instructed to respond to a given word with the first that occurred to his mind. Watt modified this form of procedure by limiting the freedom of the subject, setting

[22] "Experimentelle Beiträge zu einer Theorie des Denkens." *Archiv fur die ges. Psychol.*, 4, 1905, pp. 289-436.

before him a more definite "task." He was to respond, not with any word at all, but with a word that bore a certain kind of relation to the word given as a stimulus. The subject had six of these tasks, constituting six separate sets of experiments. They were:

1. Seek a word under whose meaning the given word is included.
2. Seek one which is included under the meaning of the given word.
3. Seek the corresponding whole.
4. Seek a part.
5. Seek a coördinate idea.
6. Seek another part of the common whole.

All the words given were familiar nouns, nearly always consisting of only two syllables, and never evidently compound words. Five hundred such words were found and printed for use in the various "tasks."

One of the principal objects of research in this study was the influence of the "task" on the whole course of events in a given experiment. In analyzing the results it appears that there are two general classes into which the experiments may be divided: (1) That in which the association is found by a simple and direct process which suffers no disturbance in its course. Verbal and visual images may be present but they help, or at least do not hinder, the finding of the required association. (2) The second class is that in which the development is complex. The subject tries two or more paths before he hits upon the one that gives the desired result.

The first class of associations is subdivided according as (*a*) visual images give rise to the association, or (*b*) a verbal image or a group of verbal images, or a condition of recollection, etc., or (*c*) no kind of imagery or media of association can be determined to show how the word spoken was found.

In reproduction of complicated development one can point to two subclasses: (*a*) The subject sought for something else, or some other idea hung in his mind without his being able to

determine just what it was. (*b*) The subject sought after some more or less definitely determined idea, but could not find it; or he had something in mind, but for one reason or another rejected it.

One way in which the influence of the task manifested itself was the mode in which it determined the means of the association. Task 3 (whole), 4 (part), and 6 (part of the common whole) tend to increase the use of visual images. Task 2 (species) tends to increase the use of verbal images, and Task 5 (coördinate ideas) tends to do away with the use of both verbal and visual imagery.

Under the head of visual images Watt brings forward some interesting facts that bear in the main upon two important problems. One is the problem with which Berkeley found so much difficulty: Are all images definite and concrete, or is there any such thing as a general image? The introspection of his subjects seems to point to the existence of what is at least a very indefinite image. We quote some examples that he has given as typical: "Hide: Image of an animal torso thickly covered with hair (very unclear). To what animal it belonged I do not know. Grain: Fleeting image of a rye or wheat field—the species was not clear. Mouth (*Maul*): Beast. Dark image of an utterly undefinable animal. It could have been an ox, or a horse, or a dog with stronger definition of the head and mouth region."[23] Watt calls attention to the fact that in this last case the image did function as if it were universal. One can, he says, maintain that it was in reality concrete and definite, but he can not prove his contention. Still, scarcely any one would wish to make such a contention. Vague, indistinct images are often like a child's drawing—they need interpretation. When we label them we know what they are, but to the uninstructed observer they may stand for a number of things. After calling attention to the existence of such "general" images, Watt then points out how illogical it would be

[23] *Op. cit.*, p. 364.

to infer from the existence of the general image the non-existence of the universal idea.

The second problem on which he touches under this heading is the position of the mental image in our mental mechanism. The mere mention of the theory of types suffices to remind us that some authors write as if certain people made use of visual images in their mental operations to the exclusion of all others because they belong to what is termed the visual type. Watt points out that the kind of image used depends upon the "task" which the subject performs.[24] By changing the task the subject passes from the visual to the verbal type.

Another point that he makes is this: The mental image is not always a merely secondary phenomenon like the illustration in a novel. It may seem at times merely to accompany the word used as a stimulus. On other occasions it is clearly the starting point for the solution of the task. In all probability the mental image never comes into the field of consciousness without exerting some influence on the development of associations. Whether by inhibition or furtherance or direct suggestion of new ideas, it has its influence on the way in which the task is performed.

In conclusion Watt sketches the outline of his theory of thought. It is an attempt to account for the flow of consciousness. He first calls attention to the fact that consciousness is not discrete but continuous. He then asks what determines the entrance of an idea into consciousness? The chief factor is the "task" that the mind is attempting to accomplish. The tendency of one idea to reproduce another is determined in a merely mechanical way by the number of times that the two ideas were perceived together in the past. But the many possibilities, the many tendencies to reproduction, are limited by the "task."

In the much discussed problem of the relation between image, word, and concept, Watt admits the existence of all three and

[24] *Op. cit.*, p. 367.

does not attempt to explain away the concept in terms of imagery or words. From the statements of his subjects it was clear that there was a distinction between the word and the understanding of the word. One could exist without the other, therefore they must be distinct. But is the understanding of the word the crowding into consciousness of a number of dark associations? One hears nothing of such associations in the understanding of the word used as a stimulus; though in seeking for the word of response such associations do occur. The burden of evidence in his experiments rather favors the view that the understanding of a word is something other than crowding in of obscure associations. But for the final determination of this point he deems that further experiment is necessary.

The following year August Messer[25] published the next study of the Würzburg School. Dr. Watt was one of his subjects.

The general purpose of the study was expressed by the author as an attempt to investigate the conscious processes that are found in simple acts of thought. The method of the experiment was based on that of Watt's work, which has just been mentioned. There were fourteen series of experiments, some of which were taken from the "tasks" invented by Watt.

1. In the first series the subject was shown a word, and his task was to speak out as quickly as possible the first word that came to his mind.

2. In the second series the task was more restricted; the word of response had to be a word representing a coördinate *object*—that is, one that belonged to one whole along with the object represented by the given word.

3. In the third, the subject was to mention a coördinate *concept*—that is, one belonging to the same genus as the given word.

4. The response was to be any adjective.

5. A characteristic of the idea designated by the given word—but not its genus.

[25] "Experimentell-psychologische Untersuchungen über das Denken," *Archiv für die ges. Psychol.*, 8, 1906, pp. 1-224.

6. Remember an object belonging under the concept of the given word and make a statement concerning it.

From the seventh to the eleventh experiment two words were shown one above the other. The upper was to be read first. The subject's tasks were:

7. Express the relation between the *ideas* designated by the given words.

8. Express the relation between the *objects* designated by the given words.

9. In the ninth series the two words were the names of celebrated men and the subject was to pass on their relative value, expressing a judgment which had real claim to objective validity.

10. In the tenth series the persons, things, or conditions represented by the given words were to be compared and a judgment expressed; but the judgment was to be one of merely subjective value and express what would be the subject's preference.

11. In the eleventh, a noun and an adjective were shown to the subjects. He was instructed to regard the two words either as a question or an assertion, and where possible to pass a judgment about them.

12. In this series the subject was shown sentences or groups of sentences and his task was to understand them and take up a position in regard to them. The groups of sentences represented logical premises and conclusions formally correct.

In the last two series of experiments the subject was shown real objects or pictures.

13. He was to speak the first word that came into his mind.

14. He was to make a statement about the object or picture.

In the first series, though no special task was given, the subject made one for himself. He involuntarily sought a word that bore some relation to the given word. In other series also the tendency was noticed to specialize still further the task assigned.

In the visual imagery of the subjects there is again found

the "general image" mentioned by Watt. This proves to be an image so imperfect that the subject can designate it only by some such word as an animal, a bird, etc. Such an image may be spoken of as general because it can stand in consciousness for an entire class.

The author also gives some account of the motor imagery that his subjects experienced during the experiments. Then after a discussion of the process of association he passes on to a problem more closely allied to our own, the understanding of the word—the concept as distinct from word and imagery.

Generally the meaning seems to come with reading the word. But even in such cases the meaning is not a constant factor. It may exist in all degrees of perfection. The word may be scarcely understood at all. It may be perceived, but merely as a sound without meaning. Or the understanding may come partially with reading and take some time to grow. This latter form leads up to the case in which there is an actual separation between the perception of the word and the apprehension of its meaning. The conditions for the separation of the word from the apprehension of its meaning are as follows:

1. The strangeness of the word.
2. Incorrect reading of the word.
3. Equivocal character of the word.
4. Imperfect knowledge of the language.
5. Number and length of the words.
6. The occurrence of a purely automatic reaction on the basis of verbal association, *e.g.*, *Laut-Schall*, *Haustier Maus*.
7. Fatigue.
8. Excitement.

The "meaning" of the word was often something that the subjects found it difficult to explain. It was frequently expressed by such an expression as "I knew what was meant." The subjects were sometimes enabled to analyze this abstract "meaning" a little further. "The understanding of the word existed in the consciousness of that general sphere to which the word belonged" (p. 77). One of the subjects expressed it

as "the consciousness that something appropriate could be associated." Sometimes the "sphere"-consciousness is identified with the generic idea to which the object belongs; again, with the entire domain in which an object belongs. For example, Subject 2 with the word of stimulus, "Hegel," said: "It seemed to me at first as if the word were 'Hagel.' As soon as the auditory image of '*e*' sprang into consciousness, there came a direction toward the History of Philosophy."[26]

At times the "sphere" of consciousness was an emotional element or word, or something similar. Again in the process of understanding, there was a consciousness of synonymous words or related objects, or some prominent characteristic of the thing represented by the word of stimulus. Sometimes the word instead of being understood in a general sense was taken in a special one, as where the word "garden" aroused the idea of a garden around a former home of the subject's family (p. 82). From all this it seems to the author extremely probable that in the process of understanding a word we have to do with phenomena of association and reproduction.

What part, if any, has the subject's imagery in his understanding of a word? The more perfect the imagery the less does it seem to cover what is meant by the general significance of the word. But the more schematic and faded the imagery, the less does it differ from the "meaning."[27] More important than the relation between the clarity of the image and the meaning, is the question: To what extent is imagery necessary to the signification? And here he says there is not one single example from which it is clearly evident that the understanding of the word was dependent on the awakening of a visual image. The most that can be said is that in a few solitary instances it was recorded that with the help of a visual image the meaning became clearer or more precise. But in the further progress

---

[26] Page 79.

[27] From what follows it is evident that the author does not mean to suggest that the meaning is nothing but faded imagery. The imagery fades into nothing, long before it gets anywhere near the "meaning," which may at times be clear without imagery.

of the experiment the subject's imagery plays an important part in the solution of the task. As to the understanding of the word of response, it often takes place before the subject can express his meaning, and when the word is found it does not always express fully the subject's mind. Sometimes too, the word of response is uttered before its meaning is understood.

The further sections of this work on the psychology of judgment, etc., are more remotely connected with our problem. The more kindred section on "Begrifflichen und gegenständlichen Denken" confirms still further the distinctions between word, image and concept.[28]

In immediate connection with the work of Watt and Messer is that of Dr. Schultze.[29] The foundation for his analysis is daily observation confirmed by his own experiments and those of others. His own experiments at the time of this article were to appear shortly under the title of "Beitrag zur Psychologie des Zeitbewusstseins."[30] He was subject in Messer's experiments and among his own subjects he numbered Külpe, Watt, and Messer.

His own work claims to be in the domain of descriptive psychology. His first problem, and the one with which we are concerned, is this: In the classification of mental processes is it justifiable to make a distinction between the sensible appearances of things and thoughts (*Erscheinungen und Gedanken*)? Originally he answered this question in the negative, but he was forced to give up this position on approaching the problem from the experimental point of view. The relinquishment of the old position seems to have required some effort, for he

---

28 For a criticism of the technique in Messer's experiments, cf. E. Meumann, "Ueber Associationsexperimente mit Beeinflussung der Reproduktionszeit." *Archiv für die ges. Psychol.*, 9, 1907, pp. 117-150. Messer replied in his article, "Bemerkungen zu meinen Experimentell-psychologischen Untersuchungen über das Denken." *Archiv für die ges. Psychol.*, 10, 1907, pp. 409-428.

29 F. E. O. Schultze, "Einige Hauptgesichtspunkte der Beschreibung in der Elementar-psychologie. I. Erscheinungen und Gedanken." *Archiv für die ges. Psychol.*, 8, 1906, pp. 241-338.

30 Cf. *Archiv für die ges. Psychol.*, 13, 1908, pp. 275-351. See especially Sec. 11, pp. 329-333.

writes: "It cost me a great resolution to say, that on the basis of immediate experiment, appearances and sensible apprehensions (*Erscheinungen und Anschaulichkeiten*) are not the only things that can be experienced. But finally I had to resign myself to my fate" (p. 277).

His reason for doing so was that the data of appearances did not exhaust the content of experience. There are marked differences between appearances and thoughts. Appearances are apprehensible by the senses (*anschaulich*) but not so thoughts. Appearances are more or less localized. When there comes a pause in any series of appearances, during that pause we are conscious indeed of various sensations from the organs of the body—but is the consciousness of the pause the perception of such sensations? When there comes a blank over the mind, what is it that is lacking—sensation or thought? Thought. Thoughts are as much a matter of immediate experience as our sensations. Thoughts are not to be explained in terms of imagery. Thought can be perfectly clear and certain but the accompanying imagery is of various degrees of clarity or is altogether lacking. Thoughts are not feelings. (1) Because we can pass judgment upon matters of feeling without actually experiencing the slightest tremor of an emotional state. (2) We can experience feelings without any intellectual state connected with them, as for example, in certain unwarranted and inexplicable emotional states. (3) There is the same independence between the clearness and importance of thought and feeling in our mental states as there is between thoughts and images.

What then is our act of thought? Not the sensations that were active in the process of its acquisition. For we make frequent use of abstract concepts but seldom in connection with these concepts do we use the definitions and sensations necessary to their original formation. No sensation can conceivably exhaust all the characteristics of the concept. Concepts then are not sensations, not mental images, not feelings. They stand apart by themselves as special factors of our mental life.

The work of Watt, Messer, and Schultze was continued by Karl Bühler.[31] He thought it advisable to study the process of thought with materials which offered far more difficulty than the comparatively simple tasks of Watt and Messer. Accordingly such questions as the following were proposed to his subjects:

"When Eucken speaks of a world-historical apperception do you know what he meant thereby?" The subject had to answer with a simple yes or no, and then give an account of all the mental processes he experienced in arriving at his answer.

In a section on the Elements of our Mental Life of Thought he propounded the question—what are these elements, and which among them is the real bearer of the process of thinking? From the protocol of his subjects there is one group of mental processes that may be easily characterized—the sense imagery, whether visual, or auditory, or sensomotor. To this may be added the consciousness of space. There are also feelings and such states as doubt, astonishment, etc. But this is not all. The most important phenomena do not fall in any of the above categories. There is something else that possesses neither the qualitative nor quantitative characteristics of the senses. These elements of our mental life are what the subject characterized as "the consciousness that," etc., or more properly and frequently as his 'concepts' (*Gedanken*).

Do we think by means of imagery or by concepts?

The answer, based upon the subjective analyses given by his subjects, is that "what enters into consciousness so fragmentarily, so sporadically, so very accidentally as our mental images can not be looked upon as the well-knitted, continuous content of our thinking" (p. 317). Concepts then, not images, are the essential elements of our thinking.

What then is the concept? Not an image nor a series of images, nor the relation to a series of images. The concept is a unit, a mental element, the ultimate result of the analysis

[31] "Tatsachen und Probleme zu einer Psychologie der Denkvorgänge." *Archiv für die ges. Psychol.*, 9, 1907, pp. 297-365; 12, 1908, pp. 1-92.

of thought. As seeing is related to a sensation of sight as "sensing" to our sensations, so is knowing related to our thoughts. "Knowing" is distinct from "sensing." It may be accompanied by sensations but cannot be supplanted by them. Word imagery does not give us the signification of words. "A meaning can never be imaged but only known (Eine Bedeutung kann man überhaupt nicht vorstellen, sondern nur wissen)."

The solution of the task is not accomplished by a single series of concepts. Between concepts there goes on a great deal of thinking—the consciousness of the task to be performed—the relation of the given concepts to others and to the task. The general consciousness of the task and the consciousness of manifold relationships constitute a kind of setting or background in which special concepts appear.

The understanding of words and sentences "is nothing less than a conscious logical relation, which brings into consciousness the connection between the thought to be understood and one already known" (12, p. 13). In many cases understanding took place by the entrance into consciousness of a more general concept, and thereupon the subject knew how and why the idea before him belonged under that concept. The mere entrance into consciousness of the more general concept does not seem to suffice, but it must be perceived as bearing a relationship to the problem before the subject. Sometimes the thought that the given idea suggests is not a more general one, but one which the subject perceives to be identical with the given thought. Sometimes the given sentence is understood by its suggesting a thought that would prove it.

The analogy, between the process of understanding a sentence and the process of perceiving a geometrical figure, will be seen at once by comparing the above analysis of Bühler's work with our own section on the process of perception.[32]

The division of Bühler's work entitled "Ueber Gedankenerrinerungen" is of great interest and value in the study of memory, but bears less directly on the general problem before

[32] Below, pp. 127-139.

us. It tends to establish more and more conclusively the existence of an imageless process of thought—not, of course, directly and *ex professo;* but still, as Bühler's study unfolds, the possibility of accounting for thought by imagery decreases.

Shortly after the first section of Bühler's "Tatsachen und Probleme" had appeared, Wundt published[33] a criticism of the methods of the Würzburg School.[34] He summed up (p. 358) the chief points of his criticism as follows:

(1) "The 'question experiments' are not experiments but self-observations under disadvantages. Not one of the requisite conditions for psychological experiments is found in them—but they rather exemplify the very opposite of these conditions.

(2) "Among the old forms of self-observation they represent the most imperfect; they occupy the attention of the observer with an unexpected, more or less difficult intellectual problem and demand of him that over and above this he should observe the behavior of his own consciousness.

(3) "In both forms of its use the method of questioning is objectionable: As a question before the experiment it places self-observation under the very unfavorable condition of the pressure of examination; as a question after the experiment it opens door and gate to the disturbing influence of suggestion. In both forms it is most seriously prejudicial to self-observation by the very fact that the subject who must observe his own self is himself the object of inspection.

(4) "The representatives of the '*method of questioning*' place themselves above the time-honored rule that in order to solve complex problems one must first be familiar with the simple ones that the former suppose. As a consequence they confound attention with consciousness and fall into the popular error of believing that everything which transpires in consciousness can be followed out without more ado in self-observation. The latter

[33] "Ueber Ausfrageexperimente und über die Methoden zur Psychologie des Denkens." *Psychologische Studien*, 3, 1907, pp. 301-360.

[34] For Bühler's answer see *Archiv für die ges. Psychol.*, 12, 1908, pp. 93-123. Wundt replied to Buhler in this same *Archiv*, 11, 1908, pp. 444-459.

ground alone would sufficiently explain the bootlessness of the question experiments.''

If then the question experiments have not proved that thoughts are not images, but have proved nothing, how then are we to go on about the study of our processes of thought? Wundt outlines the method as follows:

(1) Self-observation under favorable conditions of solitude will teach:

(*a*) That thought precedes the language by which it is expressed.

(*b*) That this thought is made up of (α) feelings that are adequate to the character of the thought and also (β) single fragments of images and words which suddenly come into consciousness and as suddenly disappear. These images seem to have been inhibited by the unfavorable conditions of the question experiments.

(2) The confirmation of the results of self-observation is to be sought in the experiments on association which show the tremendous importance which feelings have in such processes. Wundt refers to the discussion of ''idea feelings'' in his *Physiologische Psychologie,* in which it is maintained, and perhaps proved, that in the development of a complex idea feeling often precedes imagery. From such experiments one may conclude that very faint ideas can betray their presence by very clear feelings; and it would be far better to speak of an unconscious substrate of ideas or even refer the total idea to this sphere of the unconscious than to talk of ''thoughts'' and the revised Aristotelian concept of imageless ideas. But the experiments on the compass of consciousness point to a gradation from the clearly conscious to the dimly conscious, and finally to a distinct break between the conscious and the unconscious. Consequently the partial elements of an idea are not to be referred to the unconscious but to the subconscious. They are elements in one complex process which is bound together in a single conscious whole.

A ''thought'' therefore in the Wundtian sense is a complex

of images and the "adequate" feelings which are involved in their conscious unity. These feelings are combinations of his six fundamental feelings which come together to form ever higher and higher complexes. In each complex there is a total feeling peculiarly characteristic of the complex and qualitatively different from the elements that constitute it. The total feeling of the idea is ultimately analyzable into the six fundamental elements of feeling—and the final product of their combination is adequate to the character of the thought.[35]

Somewhat later E. von Aster, of Munich,[36] undertook a criticism of the line of study which culminated in the work of Bühler. The chief point of his criticism is that Bühler's subjects, in giving an account of their so-called concepts, were not describing actually present mental states, as a man who describes a visual scene, but they were making mere declarations concerning something which they had indeed experienced, but whose real nature remained to be explained. He himself leaves the problem of the nature of our "thoughts" to future research. His own opinion seems to be that our thoughts are in some manner composed of sensations and mental images and are not mental processes different from the currently recognized elements of our mental life.

A little later there appeared a criticism by E. Dürr,[37] one of Bühler's own subjects. He finally came to an opinion which takes on very closely the form of von Aster's objection.[38]

The designation of a mental process as a thought is by no means a description of the character of the thought. The main issue between Dürr and Bühler is in the analysis of the

[35] This analysis is based not merely on the article in the *Psychologische Studien,* but also on various portions of Wundt's *Grundzüge der physiologischen Psychologie.* For a criticism of Wundt's opinion see the last chapter of the present monograph, pp. 184-187.

[36] "Die psychologische Beobachtung und experimentelle Untersuchung von Denkvorgängen." *Zeitschrift für Psychologie,* 49, 1908, pp. 56-107.

[37] "Ueber die experimentelle Untersuchung der Denkvorgänge." *Zeitschrift für Psychologie,* 49, 1908, pp. 313-340.

[38] Bühler in his answer denied that Dürr's objection was the same as von Aster's. Bühler, "Zur Kritik der Denkexperimente." *Zeitschrift für Psychologie,* 51, 1909, p. 118, note 1.

"thoughts" to which Bühler's experiments had given so much prominence. Dürr would be far from agreeing with von Aster that our thoughts are ultimately reducible to sensation and mental imagery. Dürr's point of view can best be expressed in his own words:

"Bühler expressly stated that thoughts are not mental images (*Vorstellungen*) and that they have nothing in common with sensations. Now, the next question that arises is this: In our representative mental processes is there not something besides sensation; and if so, what is the relation of our thoughts to this plus?"[39]

Dürr thinks that along with our sensations there is our consciousness of time and space, of identity and similarity, etc. These things are not sensations or reducible to sensations. They might all be classed under the expression "consciousness of relationship," and this it is that will prove to be the ultimate analysis of thought.

In February, 1907, there appeared in the *Psychologischen Studien* a long article, "Ueber abstrahierende Apperzeption," by Kuno Mittenzwei."[40] It was an attempt of the Leipzig School to enter the field in which the ground had already been broken by the men of Würzburg. Mittenzwei preludes his experimental work with an historical account of the problem of abstraction from the days of Socrates to modern times. Between this historical account and his own experimental work there is no very close connection.

There are two distinct parts of the experimental work. In the first set of experiments the subject was required to direct his attention to a single circular disk (in reality the opening in an iris diaphragm). The disk was exposed twice in each experiment and the subject was required to tell what difference

[39] Page 326. I have taken some liberty in translating this last sentence. But the terminology I have chosen will, I think, give a true representation of the author's mind to English readers. The original is as follows: "Nun liegt doch die Frage nahe: Gibt es im Vorstellungsleben nicht noch etwas ausser den Empfindungen und wenn ja, wie verhalten sich die Gedanken zu diesen plus."

[40] *Psychologische Studien,* 2, 1906-7, pp. 358-492.

there might be in the size, position, or brightness of the disk in the two exposures.

In the second set of experiments the subject was called upon to observe a group of six disks, any one of which might undergo the above mentioned changes.

One is struck with the glaring difference between the task of the subject in these experiments and that of one who in real life forms what is termed an abstract idea of a group of objects. Mittenzwei's subjects had to look for a difference and neglect identity, whereas in abstraction one usually neglects all differences and finds identity. Hence, after reading the long dissertation on abstract ideas and having the appetite whetted for an experimental treatment of an old metaphysical problem, one is sadly disappointed to find that the author seems to have missed his problem. Instead of the question of abstraction he is really dealing with the perception of differences. But in spite of this serious defect Mittenzwei's experiments are not without value. One interested in the theory of spatial perception would find a very suggestive line of experiment. The problem of apperception is also helped along, even though the apperception is not —strictly speaking—that of abstraction in the logical sense of the word.

In the first series of experiments Mittenzwei measured the threshold for the perception of change,—A. Of size: Enlargement, reduction. B. Of position: Right and left, up and down. C. Of brightness: Increase, decrease.

For each of these changes he obtained two values: (*a*) one in which the subject was forewarned what change would take place, and (*b*) one in which the subject was not warned what kind of change to expect. In all changes except that of enlargement the threshold obtained when the subject was forewarned was smaller than when he was not warned. In the "enlargement" series it made no difference in the threshold whether the subject was or was not forewarned.

In the second series of experiments any one of six disks might be changed in size, position, or brightness.

It is here that the author stumbles upon some stages of development in the perception of difference. He does not name these stages, but, as the body of this work will show,[41] some of the points to which he calls attention as general phenomena are the same as certain stages that our own experiments revealed in the process of perceiving identity.

Under the name of "*der veränderte Gesamteindruck*"[42] Mittenzwei speaks of a perception of change without any knowledge of just what particular in the object was varied. The best description of this phenomenon is given in the subject's remark, "Das Objekt ist verändert, aber ich kann nicht angeben wie."

The author asks himself the question, how can such an indeterminate judgment be caused by such a particular and determinate change? This he explains by pointing out that:

(a) The second impression is involuntarily assimilated to the first, and

(b) The actual concrete change is often forgotten. Good observers have remarked, "Ich habe die Veränderung eben gehabt, aber ich habe sie schon wieder vergessen."

Under the heading of "*Partiell bestimmte Verschiedenheitsurteile*" the author describes what are really stages in the perception of difference that are a little more developed than the general impressions of change. The subject was required to give information on two points: (*a*) What was the nature of the change? (*b*) Where was it located? The determination of the location of the change comes first in the order of perception. The evidence for this lies in the fact that the erroneous or indeterminate judgments about the place of change are rare, while they are much more frequent in regard to the kind of change.

It is interesting to note how psychologically similar are the processes of perceiving identity and diversity. This will be apparent at once by the comparison of Mittenzwei's results with those reported below.

41 Cf. below, Section III, 2, pp. 127-139, more especially p. 129 ff.

42 Page 459.

The opposite of Mittenzwei's problem was taken up by A. A. Grünbaum under the title: "Ueber die Abstraktion der Gleichheit."[43] Historically it is connected with the Würzburg monographs and also with the first experiments of our own work. Grünbaum became acquainted with the method of research adopted in the present piece of research during the winter semester of 1904-5, when he was one of my subjects at the University of Leipzig. He has modified and developed the method, and for some purposes improved it. Instead of a series of exposures, each lasting but a fraction of a second, he exposed simultaneously two groups of figures for a period of three seconds. The subject was instructed to look for identical figures in two groups, thrown by a stereopticon upon a screen 4.25 m. from the subject. He was not required to fixate any point, but to distribute his attention equally over the entire field. After finding the identical figures, the subject was then to take notice of the others. After the time of exposition (3 s.) was over the subject was called upon to draw all the figures remembered, but especially the identical figures common to the two groups. After drawing what could be remembered, the subject was again shown the groups just exposed and was called upon to indicate the figures he actually recognized. The seeking and reproduction of the identical figures was termed the primary task, the noting and recognition of the remaining figures, the secondary task.

In reporting his results the author starts with the preparation of the subject for the task set before him and follows on down to the final determination of equality. The preparation of the subject for his task consisted in the picturing of a kind of frame in which there often flitted in and out vague figures. Two of the five subjects paid attention to some kind of sensory aid in their preparation. Three looked rather to the end before them and thus performed their task better than those looking to the means.

[43] *Archiv für die ges. Psychol.*, 12, 1908, pp. 340-478.

The process by which the task was performed manifested eight more or less distinct forms.

1. The method of exclusion.

The subject looks at one figure and then seeks one like it in the other group—not finding it, he takes another figure, and so on till he discovers the figure that is in both groups.

2. Successive comparison without accentuation.

The subject looks first at one group and then at the other until he recognizes one figure as having been seen before. Before recognition the common figure does not require any special prominence over the other figures.

3. Successive comparison with simple accentuation.

This method is the same as that of number two, only that before the determination of identity one figure suddenly becomes prominent—is accentuated in a characteristic way which can only be fully understood by one who has taken actual part in the experiments.

4. Successive comparison with accentuation and a realization of the task of the experiment.

In the former method the prominence of one figure seemed altogether independent of any idea of its being the one common to each group. In this method there is indeed no conjecture that the prominent figure might be the common one, but the subject is spurred on by something which one cannot express, except by some such words as the "point of view of the task before him."

5. Successive comparison with accentuation and the conjecture of identity.

This form of procedure is but a step removed from the last. With the perception of the prominent figure is united the conjecture that this may be the common one.

6. Rapid succession with accentuation of both identical figures.

In this form one figure is noticed and suddenly the other springs into prominence, sometimes so suddenly that the subject can not say but that the two figures were noticed simultaneously.

7. Simultaneous perception of the two figures.

In this form both identical figures are noticed simultaneously, and as a rule, none other.

8. Intuitive perception.

In this form the subject perceives one figure and knows at once that it is one of the identical figures without having seen the others.

Some of these divisions represent different methods of procedure, others are probably stages in one and the same method. The author, however, does not bring out this distinction. The intuitive method would have appeared less mysterious had Dr. Grünbaum pushed the inquiry a little further and taken into consideration our subconscious or unanalyzed mental content.

The primary task of the subject is the perception of a figure common to the two groups. In the fulfillment of the primary task it is interesting to notice the way in which the subject falls short of perfection in his reproduction of the common figure.

1. Instead of the perfect and complete form he will often give one that is schematically correct.

2. The subject will often draw a part of the figure and will know that something is lacking, but will be unable to supply it.

3. The correct form will be changed, but still remain recognizable. The most interesting case of this kind is what Grünbaum has called "mirror-drawing." The figure is drawn as if from its reflection in a mirror.

The success with which the primary task is accomplished decreases with the increasing number of figures in the groups. But the rate of decrease is not constant. It reaches a maximum in going from four to five figures in a group, and then rapidly declines.

The secondary task consisted in the reproduction of all the figures that could be remembered after drawing the common figure. The greater the number of figures in each group, the greater the number recalled. The ratio of figures remembered to those exposed decreases as the number of figures exposed increases. The author compared the fulfillment of the secondary

task in recognition by the method of successive perception, with and without accentuation of the figures. It appears that the accentuation of one figure during the process of perceiving identity lessens the number of figures that are remembered over and above the common element. From this it would seem that with the accentuation of one figure the negative process of abstraction from the surrounding figures is already begun.

In connection with this conclusion is the evidence that the secondary task is fulfilled better when the primary task is not accomplished—or, in other words, the perception of the common element tends to obliterate the surrounding figures.

From the experiments of Grünbaum it would seem that the process of abstraction is brought about by an apperceptive accentuation and separation of the common element. On the other hand, the surrounding figures are forced into the background and lose something of their conscious value.

Here it may be well to append the abstract of the early experiments of this study which appeared in the report of the Fifth International Congress of Psychology, held at Rome in 1905.

THE PROCESS OF RECOGNITION.

"The problem of research undertaken in this set of experiments may be briefly stated as follows:

When a series of groups of figures (*e.g.*, a square, triangle, etc.) is represented to a subject and in each group one figure is always repeated, what mental process will be involved in recognizing that a figure has recurred in the series? It was not required of a subject that he should be certain that a figure recurred in each group, but only that he could say with certainty that some figure had been repeated.

Hitherto the problem of recognition has been mainly confined to the comparison of the sensations or perceptions of distance, etc., which the subject was to judge of as the same or different. But to surround the elements to be recognized with varying sensations brings the problem of recognition nearer to the conditions of real life and also enables us to approach by

experimental methods somewhat closer to philosophical problems with which metaphysics has long been engaged.

In order to simplify the mental processes involved as much as possible the time of exposing a group of figures and the interval between exposures were both limited to a fourth of a second. A longer interval in either case would have given time for reflection, comparison, acts of the will to remember certain figures, and other rather complicated mental processes. The shorter interval eliminated in great measure these processes, for before there was time for reflection or comparison a new group of figures was represented.

The mechanism by which the expositions were given consisted of a metronome and Dr. Wirth's memory apparatus.

When a subject had perceived that a figure had been repeated, he was asked to give an account of the development of this process of recognition which he had just experienced. The subjective analysis thus obtained was in later experiments tested by limiting the number of expositions, so that the series of exposures ended before the observer had arrived at complete certainty. He was then asked to give an opinion and describe his state of mind. A control over the experiments was always kept by introducing from time to time a series of exposures in which no figure was repeated.

The following steps (naturally with various graduations) in the process of recognition were noted by means of this method:

1. An intimation of some kind of a figure being repeated without any knowledge of its form.

2. An intimation of some kind of a figure being repeated and a very imperfect idea of its form (*e.g.*, a dark spot, a cloudy spot which afterwards cleared up, an unsymmetrical figure, etc.).

3. Certainty that a figure is repeated but a clear image of only a part of the figure.

4. Certainty that a figure is repeated and a clear image of the form.

These steps seem to be but points in the more common and fuller order of development.

The subjects often remarked, when they first saw the common figure, it had already a tone of familiarity.

It sometimes happens that the blind intimation of a figure being repeated increases to certainty without any image of the figure being formed. This was especially the case when two figures were alternately repeated in a series of exposures.

The perception of the figure repeated has a tendency to force the other figures out of consciousness. *E.g.*, Subject K, in experiments where no figure was recognized as repeated, could afterwards draw the following numbers of figures as remembered: 3, 2, 2, 4, 4, 2, 1, 3. When, however, he had perceived a common figure he could draw as remembered only 0, 0, 0, 1, 0, 0, 0, 1.

It would thus seem that under the simple conditions of the experiments the progress of recognition is by no means a simple act, and that the formation of a mental picture is not the only or the most important factor.''[44]

[44] *Atti del V. Congresso internazionale di Psicologia tenuto in Roma, dal 26 al 30 Aprile, 1905,* pp. 286-287.

II.

THE METHOD OF RESEARCH.

1. The Problem and the Experiments.

It is very seldom, if at all, possible to reproduce in the laboratory the exact conditions of real life. Most of our experiments can only approximate more or less closely the actual occurrences in the external world. This is not, however, an insurmountable difficulty for experimental psychology. We have not one mind for the laboratory and another for the world. The same mental processes that take place in the world are observed in the laboratory, but under different conditions. The change in conditions is in the direction of greater simplification. The mental process of the laboratory is, as it were, a purified product and its true properties can therefore be more easily determined. The process of abstraction as studied in our experiments is certainly not the same as that of ordinary life. But it involves those very elements which are essential to the extra-laboratory mental operation. For this reason the present work is truly a study of abstraction.

The first method of experiment that I conceived of would have reproduced in the laboratory, almost exactly, the process of abstraction as it often occurs in actual life. It would have consisted in presenting to the subject a series of sentences, each containing a common idea. The subject's task would be to find the common idea, and report the mental processes he experienced in doing so. This method, however, is hard to bring under experimental conditions. I then thought of exposing to a subject a series of drawings. Each drawing would represent a single object, *e.g.*, a series of net-veined or parallel-veined leaves. The subject's task would be to pick out the common characteristic. Dr. Thorndike, of Columbia University, recently told me that he had thought of the same experiment, and suggested

exposing a series of bilaterally symmetrical figures. By a little ingenuity a sufficiently complete material could be worked up, and this method of experiment would afford the opportunity for a valuable piece of research.[1]

But neither of these methods of experiments is as simple as the one finally adopted, which, though farther away from the actual conditions of outside life, still involves the essential factors of the process of abstraction. It may be well to note here that, when in the future I speak of "the process of abstraction," I mean, of course, the process as it existed under the conditions of these experiments. The analysis which results from our experiments is applicable to the real process of actual life only in so far as it appears that factors are analyzed in the laboratory which do occur in the more complete processes outside the laboratory.

The method I finally decided upon may be described as follows:

Let a group of geometrical figures stand for a group of qualities. Such a group has not indeed the unity that we see in the qualities of any object. However, when one is allowed but a single glance for one-fourth of a second at such a group, it really approximates the desired unity much more closely than would be expected. Let us expose in succession to a subject a series of groups of figures. In each group let there be one common element that constantly recurs. Of all the other elements that go to make up the groups of the series, let no two be the same. Representing our geometrical figures by letters, the following will give some idea of what is meant. Let us for example take a series of five groups with three figures in each group. This would be represented by:

1. A B C. 2. D A F. 3. G H A. 4. A I J. 5. K A L.

There is one element common to each group, and this one element is the common quality that is to be abstracted. The

[1] I wish to express here my indebtedness to Dr. Thorndike. It was his discussion of a paper I read in his class of Educational Psychology at Columbia University which was, although only indirectly, the first stimulus to the present work.

letters of the alphabet might serve for such an experiment were it not that their number is altogether too small. Instead of letters I used a specially designed set of figures.

Fig. 1.—The originals used were each about $1\frac{3}{4}$ times as large as in this reproduction (see Fig. 2, p. 122 for actual size). The numerals at the side and bottom are merely for convenience of reference in the text. In these references, the first numeral (in lighter face) indicates position along the axis of abscissas; the second numeral (in heavier face) indicates position along the axis of ordinates. Not all of the figures were actually used. On account of their very evident associations the following were excluded: (1, 2)·(2, 2)·(5, 2)·(6, 2)·(11, 2)·(14, 5).

The figures possess one great advantage. On account of their strangeness, the process of perceiving them goes through a longer course of development and thereby one is enabled to detect points which it would otherwise be impossible to notice, or at least could be obtained with difficulty and uncertainty.

The groups of figures in the actual experiments (see cuts on pages 122 and 123) contained five figures instead of three. This drew out to some length the process of isolating and perceiving the common elements, thereby allowing a better opportunity to observe the development of the mental processes involved.

2. The Apparatus.

For the experiments in Leipzig, Wirth's memory apparatus with rotating disk was used. At the University of California I used Ranschburg's memory apparatus. Each performs the same function and the same disks may be used in either apparatus. Each rotates a disk and exposes suddenly a small surface and as suddenly removes it from view. In this experiment a group of five figures was exposed for a quarter of a second and then a blank space for a quarter of a second and so on till the series of twenty-five exposures came to an end or as much of the series was used as necessary for the experiment. It was at Professor Wundt's suggestion that I used this short time of exposure and interval between exposures. It tends to reduce the experiment to simpler and therefore more constant conditions by cutting out to a large extent such variable factors as reflection on what was seen, comparison, and voluntary association.

To beat time I have used both the metronome and the time sense apparatus, but generally the former, which is sufficiently accurate for the purpose. Care was taken to keep both these pieces of apparatus out of the room in which the observer was seated.

3. Instructions to the Subject.

The subject was instructed to look for the repetition of some figure and to turn a switch, which stopped the rotation of the disk, as soon as he was certain that he had seen some figure repeated. It was not required of him to see this figure in each group as it passed by, but merely to be sure that he had seen some figure twice. He was told not to wait until he knew all

about the figure but only to make sure that one and the same figure had occurred more than once. He was required at the end of the experiment to describe his state of mind during the experiment, and especially to tell what it was that he first noticed.

4. CLASSIFICATION OF THE EXPERIMENTS.

It soon became apparent that the method offered exceptional advantages for a genetic study of the process of abstraction. In handling the results and attempting to reduce them to some kind of order, the complex nature of abstraction became evident. And at the same time its analysis was greatly facilitated. Our five figures were found to constitute something of a unit which underwent a real process of breaking up. This was evidenced by the fact that the elements of a group have a different mental value after the perception of a common element than before.[2] Before the common element is noticed, the figures of any group have a tendency to persevere in memory, which varies with the focality of their perception[3] and with their own inherent attractiveness.[4] After the common element has been perceived the tendency of the other figures to persevere in memory is greatly reduced. The group is no longer what it was before it was broken up. This breaking up of the group is one of the several processes which form the mental complex that we call abstraction. The breaking up of the group is intimately bound up with the perception of the common element. Perception then is another factor in abstraction. The figure perceived is remembered and recognized again upon its recurrence. We have then four points in our preliminary analysis of abstraction: (1) The breaking up of the group; (2) The process of perception; (3) The process of memory; (4) The process of recognition. Each one of these has been made the object of experiment and form the four main headings in our experimental data.

---

[2] See below, pp. 124-127.

[3] See below, pp. 158-159.

[4] See below, pp. 122-124.

These experiments were commenced in Wundt's laboratory at the University of Leipzig. They were afterwards continued at the University of California. My thanks are due Professor Wundt for his kindly and valuable suggestions as to the method of experiment and also to Dr. Felix Krueger for his constant interest and assistance while I was working at Leipzig. I wish also to express my indebtedness to Professor Stratton and Dr. Wrinch, with whose valuable coöperation the experiments were conducted at the University of California.

The subjects who took part in the experiments were Miss Ball (B), Dr. Bessmer (Be), Dr. Brown (Br), Herr Blosfeldt (Bl), Miss Deamer (D), Herr Grünbaum (G), Dr. Krueger (K), Dr. Moore (Mo), Miss Mower (Mw), Miss Ross (R), Professor Stratton (S), Professor Eustachius von Ugarte (U), Mr. Wabeke (W), Dr. Wrinch (Wr), and Herr Ziembinski (Z).

III.

EXPERIMENTAL RESULTS.

1. The Analysis of the Groups.

(a) Isolation of the Common Element.

In abstraction some element or characteristic is always picked out from a group and is recognized as identical with that which was found in another group. In our experiments this element was the repeated figure. We may ask what is it in any element that accelerates the process of its isolation and perception? The answer as one might expect is—whatever attracts attention to the element. This may be the pure accident of its focal

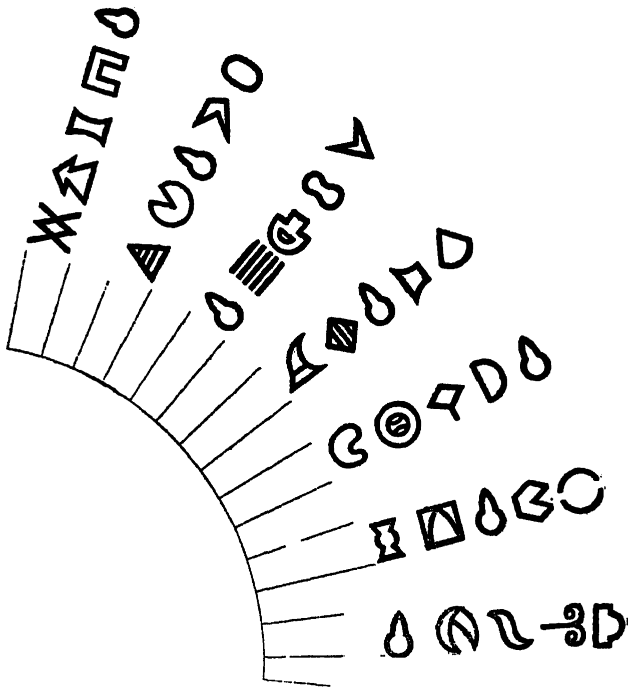


Fig. 2.—Showing grouping of 'elements' for actual display upon the disk, the common element following in the order 1, 3, 5, 3, 1, 3, 5.

perception. It may be the fact that it is rather larger than the other figures or blacker or more open.

Small but symmetrical figures, *e.g.*, 4, **12**, and 5, **12**, in Fig. 1, seem to pass by easily without being noticed. Another drawback is apparently the complication of the figure. This, however, is probably only apparent. The subject involuntarily waits to be informed about the complicated figures. Complication is, in itself, an advantage because it attracts attention. But the subject waits to know just what is repeated. In spite of instructions, he cannot stop the apparatus as soon as he is sure of the bare fact that a figure of some kind has been repeated.

The attempt was made to find out whether the sequence of position had any influence in the perception of the common element. If we number each of the five positions in a group

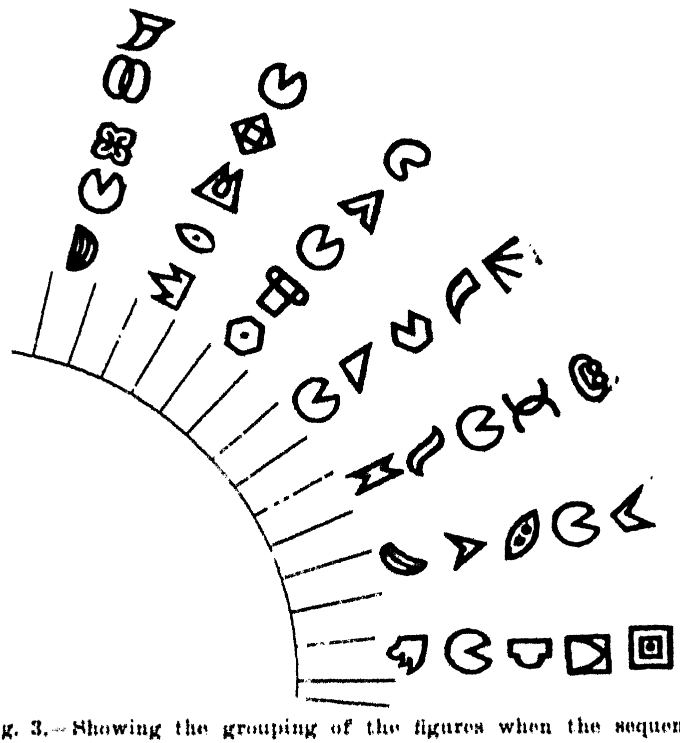


Fig. 3.—Showing the grouping of the figures when the sequence of the common element is altogether irregular.

of five elements 1, 2, 3, 4, 5, we have a means of recording this sequence of position. In the first group the common element may come in position one; in the second in two, and so on. We may have such an order as 1, 2, 3, 4, 5, 1, 2, 3, 4, 5 or 1, 3, 5, 3, 1, 3, 5 or 1, 5, 1, 5, etc., or the sequence of the common element may be altogether irregular. Whatever influence the sequence of position might have, it is so slight that it is obscured by the varying attractiveness of the figures themselves. So long as the common element does not come twice or oftener in the same position, the sequence of position seems to have but little effect. If it comes twice in the same position and the subject happens to see it, he involuntarily looks at the same place in the next exposition. In a word, then, it would seem that what was naturally to be expected is here the case. Everything that calls attention to the figure, either accidental circumstances or inherent qualities, tends to accelerate the process of its isolation and perception.

(b) The Disappearance of the Surrounding Elements.

Once the common element has been perceived, the surrounding elements are swept from the field of consciousness. They do not merely become less prominent, as one of the surrounding elements does when another is noted. They are forced into oblivion, usually complete. Rarely, one or two can still be remembered. In the passage quoted from the *Atti del V Congresso*[2] I reported that "Subject K, in experiments where no figure was recognized as repeated, could afterwards draw the following numbers of figures as remembered: 3, 2, 2, 4, 4, 2, 1, 3. When, however, he had perceived a common figure, he could draw as remembered only 0, 0, 0, 1, 0, 0, 0, 1."

This condensed account of the experiments needs explanation in order that it may be understood. I think that it was reading the account of Külpe's *"Abstractionsversuche"* that first suggested to me that I could test one of the results that he obtained in Würzburg. In his paper before the first German Congress

[2] See p. 115.

of Experimental Psychology he reported[3] that "negative abstraction has its most evident effect in the most difficult task" (p. 65). The greater, therefore, the absorption of the attention in the principal task, the less is remembered of the secondary task. It might therefore be concluded as a general law that the perception of the element to be abstracted has a tendency to obliterate the memory of the other elements. To test this result by our own method I asked the subjects at the end of the experiments to draw the figures which they remembered. Sometimes they had noticed a common element and sometimes it happened that they had seen no common element—either because there was none to see, or through some accident they failed to notice the common element that was present. Whether or not a common element is actually present made no very great difference so long as it was not noticed. I did not put this latter point to a careful test, and a common element that is not noticed at all may have some slight effect which is lacking when no common element is present. But there is a very great difference in the number of figures which can be remembered after an experiment in which no common element was perceived, and one in which the subject did see a common element. The numerals given above for subject K give the number of figures that he could draw in experiments where he had not seen, and again where he had seen, a common element. They seem to prove that the perception of a common element places the surrounding elements at a great disadvantage so far as their preservation from oblivion is concerned. But the figures as given are open to objection. It did not occur to me at the time that the series where no common element was perceived were generally longer than those where one was perceived. Hence there was a greater chance to remember more figures. However, the subjective analysis leaves no room for doubt on the matter. It is with great difficulty that one remembers the other figures after perceiving the common element. Whereas

[3] *Bericht über den I. Kongress für experimentelle Psychologie in Giessen, 1904*, pp. 62 ff.

when no common element is perceived, several figures are usually drawn readily and with ease.

However we are not left entirely to subjective analysis in the matter. Even when we take into consideration the relation of the length of the series to the number of figures remembered, we see that the memory of the surrounding elements is at a decided disadvantage whenever the common element is perceived.

The following results make this point clear:

SUBJECT K.

Common Element Not Seen.		*Common Element Seen.*	
Figures remembered.	No. of expositions.	Figures remembered.	No. of expositions.
		0	14
3	25	0	10
2	25	0	11
2	25	1	13
4	25	0	9
4	25	0	7
2	24	0	11
1	25	0	20
3	25	1	12
21	199	2	107

10.5 = Percentage of figures remembered when the common element was not seen.

1.9 = Percentage of figures remembered when the common element was seen.

SUBJECT W.

Common Element Not Seen.		*Common Element Seen.*	
Figures remembered.	No. of expositions.	Figures remembered.	No. of expositions.
		3	20
7	25	5	24
3	25	2	9
9	25	3	25
7	25	0	12
3	25	2	11
4	25	0	20
33	150	15	121

22.0 = Percentage of figures remembered when the common element was not seen.

13.2 = Percentage of figures remembered when the common element was seen.

SUBJECT G.

Common Element Not Seen. Figures remembered.	No. of expositions.	*Common Element Seen.* Figures remembered.	No. of expositions
		0	15
		4	21
2	25	0	4
4	25	0	4
5	25	0	4
0	25	1	7
5	25	0	8
3	25	0	7
6	25	0	4
3	25	1	13
28	200	6	87

14.0 = Percentage of figures remembered when the common element was not seen.

6.9 = Percentage of figures remembered when the common element was seen.

2. THE PROCESS OF PERCEPTION.

It would seem at first sight that the sense-perception of a given object is a matter which concerns, almost exclusively, the sensations involved in the act of perceiving. Suppose for instance that we have to do with the visual perception of some object. Then we can pick out the shades of brightness and the tints of color, and the spatial data given by the sensations arising from movements of the eye, and, if you will, the feelings of pleasure or dislike that may be involved. And this analysis having been completed, the task of the psychologist seems to have been done. The analysis is exhaustive and nothing more is required. This is a superficial view of the matter, but still a view which seems perfectly warranted until one seeks to find by experiment just what are the factors in the process of perception. It then appears that there are two factors. One may be termed objective. It involves the elements mentioned in the analysis just given. The other may be named subjective. This involves the correlation of the data of objective perception with that of past experience,—'apperception' in the Herbartian terminology; 'assimilation' in the Wundtian.

We may use the words 'perception' and 'apperception' for the objective and subjective factors in our apprehension of an object.

In the visual perception of an object there is one point which may be regarded as a stage of relative perfection, and that is the acquisition of a definite image. In the process of apperception there is no such stage which may be designated as perfect, nor indeed is it always easy to say whether or not the object has been apperceived at all.

For this reason we may take the acquisition of a definite image of an object as a kind of cardinal point and ask ourselves what stages of perception and apperception precede and what follow the clear visualization of the object. One of the first things which becomes apparent in going over the data of the experiments is this: Perception and apperception were intertwined in the process of apprehending the common element. Concerning the apprehension of the figures surrounding the common element the experiments give practically no data. It became possible to pick out stages in the apprehension of the common element because (*a*) the subject's attention was directed to seeing a figure repeat itself and thereby a special figure had to be looked for and impressed on the memory; and (*b*) the process of apprehension was often long drawn out, thereby giving an opportunity for the stages to be definite enough for detection.

The experiments in which the development of the knowledge of the figure was long drawn out were in the minority, and represent those cases where the process of apprehending the figure was relatively difficult. Or rather, they represent those cases where a focal perception of the common element was accidentally delayed. These are the hopeful cases for psychological analysis. Any one could look at two of our figures and tell at a glance whether they were the same or not. But in so doing he could not say with certainty just how he came to that conclusion, except that his eyes told him so. Perhaps here one really is concerned with a comparison of visual images. But even this is not clear. Suppose the rate of succeeding impres-

sions is so rapid that there is no time to stop and compare images; suppose, too, that it is not possible to see the figure at will in the focal point of vision; what then will tell us that two succeeding figures are identical? This is really what was done in our experiments. The rapidity of the rhythm of exposition, the changing position of the common element, made focal perception at will an impossibility. As a result, the process of apprehending the common element often proceeded in stages that were well marked, and thereby it became possible to analyze it. For it turns out that the apprehension of a simple figure is not itself as simple as one might suppose. Certainly there is involved in it something more than mere seeing with one's eyes.

Some samples of the subjects' introspections are given below. These data were obtained by running through the experiments and picking out what the subjects described as the first thing to be noted in their apprehension of the common element. The samples given may be considered as answers of our subjects to the question, "What did you first notice?" if we exclude as irrelevant the remarks about certain figures which attracted their attention before any idea of a common element was present.

A. DATA ACQUIRED BEFORE A CLEAR PERCEPTION OF THE FORM.

1. *Feeling Tone:*

An unpleasant unsymmetrical figure. (Bl.)

2. *Appropriate Mental Categories:*

The idea of some kind of a figure; absolutely no determinate knowledge of just what kind; a very frequent case, and one that represents the earliest stage of perception.

A horizontally lying curve.[4]

Symmetry:

Subject noticed first that the figure was bi-laterally symmetrical, and only on seeing it somewhat later did he get an idea of the form. (Wr.) A pointed symmetrical figure. (Mw.) An unsymmetrical figure. (D.)

An idea of the figure changing its position, before an image of its form. (K.)

A common element similar to the one in the preceding experiment. (Mo.)

[4] A rare instance of spatial direction being given before a clear image. The figure was (8, 13). For an explanation of this manner of referring to the figures by number, see page 118.

Familiarity or Unfamiliarity:

Idea of a common element, then of something strange. (R.) In one series of experiments, figures were introduced which the subject had not seen before. Z said that with these disks he first noticed something new, and afterwards a special figure.

3. *Partial Perception of the Figure:*

Subject knew first that a common element was present, then that it was circular in form, then he obtained a complete idea of form. (G.)

A pointed figure. (Mo.)

An open kind of figure. (Mo.)

Subject knew that the figure was round, and had an idea of about how big it was, and could not get true form. (The figure was 10, 15.) (Mw.)

A narrowly oblong figure. (Mw.)

Something with top lines crossed. On stopping the apparatus knew exactly what the figure was. The image faded and the abstraction, something with top lines crossed, remained. (Mw.)

Subject first noticed something resembling a heart, then that it was different from a heart. (R.)

"I next noticed that the figure was pointed." (R.)

At end of experiment subject had forgotten whether the figure was a circle or a polygon. (R.)

A bar in the center with some kind of curves. This the subject attempted to draw, but failed utterly to produce the figure. (R.) Subject knows that the figure has a square in the middle, but cannot place the square where it belongs. (Bl.)

At end of experiment the subject was certain of two triangles with points together, and did not know just how the other lines were drawn. (Bl.)

At end of experiment subject remembered a diamond in the center of the figure, but was able gradually to build up the figure from this one fact and draw it (3, 3) correctly. (Bl.)

Had at first the idea of some kind of a polygon. (Bl.)

Thought a dark spot would turn out to be the common element, and so it did. (Bl.)

A narrow oblong figure. (D.)

Noticed at first a point and then a square. (D.)

First notices points and then something crossed; finally obtained the true image. (Gr.)

Knows that figure consists of two triangles, but does not know where to place the corners. (Z.)

At the end of the experiment the subject knew that the figure had something round in the middle. He was able to pick out the right figure (13, 11), on another disk, when it was placed before him. (Z.)

B. Data acquired after a clear perception of the form.

1. *The idea of a figure's orientation:*

In some figures a distinct axis may easily be picked out. They are built around this axis. And according to the position of the axis the figure may be turned to the left and right, or up and down, or it may be rotated around the central point of this axis. The actual position of the figure, as determined by the direction of the axis, is what I mean by the figure's orientation. It frequently happened that the subjects were in doubt about the orientation of the figure but felt perfectly certain about the form and did have, in fact, a correct knowledge of all the details of the figure. This leads one to suppose that since the orientation of the figure is not given with the perception of the shape and details of the figure's composition, it must require for its perception a distinct act over and above the act or acts of apprehension that are necessary to acquire a knowledge of the form.

Not only is the subject often left in doubt about the orientation of the figure, but he frequently is the victim of a delusion. It is an interesting fact that the subject would often be positively certain of an orientation that was just the opposite of the true one.

The explanation of this erroneous judgment is not certain. Grünbaum has termed the phenomenon mirror-drawing (*Spiegelzeichnung*), intimating that the figure is drawn as if from its reflection in a mirror. The errors, however, are not merely such as would be caused by drawing from a reflection in a mirror. They may indeed be right and left reversals, but they may also be up and down reversals or rotations of the figure, through an angle of ninety or of a hundred and eighty degrees; or errors which would be produced by combining the above alterations of position. As a possible explanation of the delusion, I would suggest the following: The figure was seen by glancing at it from another figure. This glance involved a movement of the eye. Ordinarily the orientation of a figure is judged at leisure, by moving the eye from one part of the figure to

another. But in the rapidly disappearing figures, in our experiments, this was not always possible. The details of the form were seen first. This involved perhaps a single glance, which was an up and down movement of the eye, or a right and left movement or a rotation of the eyeball. This glance at the figure gave rise to the idea of its orientation. It was interpreted as a glance along the axis of the figure or from one point of the figure to another. There was no time to correct the first idea by a second movement of the eye. And the chance movement, in observing the figure, gave rise to the delusion of orientation.

From the fact that such delusions and doubts occur frequently, it seems clear that a true and certain perception of a figure's orientation requires a special mental act and is not ordinarily given with the perception of its form. Rarely, it may precede the full perception of the form.

The following stages of development give in brief outline an analysis of the process of perception:

The Stages of Perception.

1. The general idea of some kind of a figure being repeated. In this stage there is no definite information about the shape or nature of the figure whatsoever.

2. A more or less specialized idea of the figure. This idea of the figure may be expressed by perfectly general terms or it may be accompanied by a more or less perfect image.

3. A correct idea of the figure and clear knowledge of its shape, but doubt about or error as to its orientation.

4. A correct idea of the figure and its shape with a true knowledge of its orientation.

A pleasant or unpleasant tone of feeling may accompany any of these three last stages. I have never found it with the first.

From the above analysis the order of development seems evident. The subject does not pass from the individual to the general, from the concrete to the abstract, but just the reverse. What is offered to vision is individual and concrete enough. But what one first sees and holds on to is something that can

fit into some kind of a mental category that the figure suggests. What one sees and does not hold on to, but at once forgets, takes no further part in the process of development. The mental category may be as wide as that conveyed by our idea of 'something.' Or again, it may pick out some special characteristic of the figure. There are, indeed, two classes of incomplete apprehensions of the figure used above and spoken of as appropriate mental categories and partial perceptions. The partial perceptions noted above must not all be put down as mere incomplete images. Once a subject said that she knew the figure was made of curved lines. She had not the slightest image of any curved lines nor any idea of how they were arranged. She attempted to draw some curved lines but failed utterly to reproduce the figure or any part thereof. Had there been a mental image of any part of the figure, that part could have been drawn. But there was no image. On perceiving the figure, it called up by association the idea of curved lines. That the figure belonged to the class of curved-line figures was apprehended clearly and remembered. The image of the curved lines was not remembered.

Another instance is that of Mw above. The final result was the memory of something with the top lines crossed. On stopping the apparatus she knew exactly what the figure was. The image faded and the abstraction "something with top lines crossed" remained.

Sometimes there may, indeed, be a piece of a mental image in the mind. But frequently, perhaps generally, this is not the case. The partial memory of the figure means simply that the figure called up some such general concepts as points or angles or curves, etc. The subject remembers the fact that the sensation fitted the concept at the time of perception. The sensation may leave no image behind it, but the memory of the fact remains.

We may regard the process of perception as terminating in a mental state representative of the figure and reproduceable in memory. The case of G above may be taken as representative of a good course of development. The subject knew (*a*) that

a common element (12, **13**) of some kind was present. (*b*) He then knew that it was circular in form. (*c*) And finally he obtained the true mental image or at least a mental state that enabled him to reproduce correctly the figure he had perceived.

When the subject knew that some kind of a common element was present, he was not, of course, seeing a general idea of something with his eyes. On the contrary, perfectly individual and concrete sensations were being perceived. The process of perception was one of normal sensation, but in all probability not of focal vision. The process of apperception was the recognition of these sensations as belonging to the general concept "some kind of a figure." Later on, this concept was specialized to that of "a figure circular in form." Still later in the process of development, a mental picture was obtained representing the figure in its details.

However, *the mental image forms no essential part in the apprehension of a figure.* It is like the illustration in a book[5] which is useful but not necessary for the sequence of thought. The contention that the mental image forms no essential part in the apprehension of a figure is proved by the following experimental facts:

(*a*) Subjects Be, Bl, G, K, M, W, Z were at times conscious of a figure repeating itself before they could say anything more than that some figure was being repeated.

A figure was apprehended as repeating itself. Certainly, therefore, a figure was apprehended. But there was no image, nor any further knowledge of it than that it was some kind of a figure. Therefore the image is not necessary in the apprehension of the figure. One must not confound the visual image with the visual sensation. Without the sensations one might just as well have his eyes shut. There would be no apprehension at all, because nothing to apprehend. But in the resultant from the visual sensations, the mental image has no essential

[5] In the above statement I do not mean to call in question the conclusion of Watt about the utility of the mental image in forming associations. Mental imagery is very convenient and useful, but it is not the only element in the flow of thought, nor is it an essential element. Cf. Watt, *Archiv. f. d. ges. Psychol.*, 4, pp. 361 ff.

part because it may be lacking altogether. The residuum may be the bare fact of memory that the sensations did call up certain generalizations and did represent something belonging in their category.

(*b*) These results were not only obtained by a subjective analysis of the course of development after it had transpired; but were also confirmed by an objective test. Shortened series of expositions were given so as to cut short the process of development before it had arrived at completion. For example: Subjects who require on an average fifteen expositions to be sure that a figure was repeated, were given six or seven. They were asked at the end of the abridged series to describe their state of mind. In this way a kind of cross-section of the process of perception in its course of development was obtained, and a confirmation of the results of memory and introspection was secured.[6]

(*c*) The following series of experiments with subject Z is noteworthy in this regard. A series of disks was prepared in which there were two common elements. Common element *a* appeared in groups 1, 3, 5, etc. Common element *b* appeared in groups 2, 4, 6, etc. The two common elements were thus exposed to view alternately. The subject was told to stop the apparatus as soon as he was certain of one common element. He did not know whether or not there would be one common element in each group, or two in alternate groups, or none at all.

Experiment 1. Common Element (9, 12)–(10, 16).

Result: The subject had a feeling that some kind of a common element was present during a period in which no determinate figure had as yet been noted. After four exposures he was sure of 9, 12 and when the apparatus stopped he saw 10, 16 by accident and knew that it had been there before. The subject then drew both figures correctly.

Experiment 2. Common Element (10, 15)–(10, 14).

After ten exposures the subject drew 10, 15 and said that he had a feeling that another common element was present.

[6] For a fuller account, see below, p. 163.

Experiment 9. Common Element (5, 12)–(4, 12).

The subject stopped the apparatus after nineteen exposures and said that he was perfectly certain of some common element and that there should be two, because two certainties kept crossing each other in consciousness, but with no image. The feeling of certainty could not attach itself to any common element because before it could do so, another feeling came of another common element. He was unable to draw or tell anything about either of the common elements.

Experiment 12. Common Element (14, 14)–(13, 14).

After nine exposures the subject stopped the apparatus and said that he was certain of two common elements, but what they were he did not know. He had a feeling that there was some common element, but he could not find it. He judged that there were two because of the difference in feeling between these experiments and those in which there was but one common element in each group.

The remaining experiments of this series confirm these results.

Further experiments were made with this subject, in which disks were introduced having one common element in groups 1, 3, 5, etc. In groups 2, 4, 6, etc., there was no common element. He sometimes mistook these disks for those of two common elements, but he never said that there was a common element on a disk where there was none at all. Nor did he mistake a disk with one common element in each group for one with two common elements.

These experiments make it perfectly clear that a common element may be perceived and that, too, with certainty, while in apprehending it there is not only no mental picture left in the mind, but not even a more or less specialized general concept of its form.

From the experiments of the above sections (*a, b,* and *c,* pp. 134 ff.), the conclusion is warranted that *a mental picture forms no essential part of our apprehension of a figure.*

Taking perception as a general term to cover all the processes by which we arrive at a knowledge of the figure, we may distinguish therein the following factors:

A. The Process:

(*a*) *An objective Factor:*

The sensations to which the figure gives rise. The reception of these sensations by the mind institutes a process of apperception.

(*b*) *The Feelings:*

Here feeling is taken in its strict sense, as pleasurable or disagreeable, or a feeling of tension, etc.

(*c*) *A subjective Factor:*

The sensations are recognized as representing an object which belongs to one or more mental categories.

B. The Residuum:

(*a*) The memory of the fact that the object belongs to such or such categories.

(*b*) The mental image.

(*c*) The memory of the figure's orientation.

In the residuum, (*a*) is essential, (*b*) is not essential. I cannot have an image of a figure without at least knowing implicitly that it belongs to my mental category of figures. But the experiments have shown that (*b*) is not essential, for one can apprehend a figure without forming any mental picture thereof.

The above view of the process of abstraction is borne out in some important details by the work of Arthur E. Davies of the Ohio State University.[7] This experiment regards two facts as established.

"The first is, that perception is a mental process, not an act; and the second, that the perceptual content undergoes a growth before it can be definitely defined" (p. 189).

Our own experiments have made these points abundantly clear. He also agrees with us in a conclusion that he puts forward tentatively. "Primitive psychic material does not seem to be so much received from without, as developed from within."[8] Taking this to mean in our own terminology that in the process of perception there is both an objective and a subjective factor,

---

[7] A. E. Davies, "An Analysis of Psychic Process." *Psychological Review,* 12, 1905, pp. 166-206.

[8] *Op. cit.,* p. 200.

it must be put among the established facts of psychology; and indeed the subjective factor does have very much to do with the final product—perhaps more than the objective. The author points out three stages in the perception of the form:[9] (*a*) the perception of light. (*b*) An imperfect perception of the form. (*c*) A perfect perception of the form. That the perception of light should enter here as a stage prior to any perception of the form is indeed remarkable. The explanation, however, is to be found in the conditions of the experiment. The subject sat in the dark and the figures were illumined by a flash of light. Before any perception of the form could take place, there had to be a process of adaptation during which only light could be perceived. It is strange that the author makes no reference to adaptation as accounting for the perception of light prior to that of form. The flash of light, too, accounts for the feelings of tension, surprise, etc., to which so much attention was given by the author. However, it would be natural to suppose that a feeling of tension or excitement, or both combined in a weak emotion of surprise, might well precede the entrance of any objective perception into the field of consciousness.

The author notes that positive examples of association[10] were rare. The experience in our own experiment was that with some subjects they are plentiful enough, but rare with others.

In the following section, he seems to differ from us radically. "If therefore by association is meant the subsumption of a particular perception under a general idea or class, we do not find that such a procedure is characteristic of elementary psychic process."[11]

We take it, however, that his data on this point is negative. He did not find it to be so. And indeed he does not say that it is not the case. So, even here there is no real contradiction of results. The method used was not calculated to bring out that stage of perception which we found, in which there was no

[9] Page 176.

[10] We suppose that there is here meant association of the figures perceived with various ideas or objects of real life.

[11] Page 191.

image but only the bare knowledge of some kind of a figure. Davies exposed a figure by a momentary flash of light in a dark room. The subject was then required to give an account of his experience. There was always but a single exposure and no chance for a long-drawn-out development of the process of perception such as occurred in our own experiments. The op-opportunity for a longer process of development enabled the analysis based upon our own experiments to be more complete.

3. The Factor of Memory in the Process of Abstraction.

After the common element has been separated from the elements that surround it and perceived, it must be held in memory. The memory of the isolated characteristic or group of qualities is an essential element in the process of abstraction. Consequently it would seem desirable to investigate this process of memory as it occurred in our experiments.

There are three factors which are readily seen to affect the process of memory:

(*a*) The method of memorizing—visual or motor, or whatever method may be used.

(*b*) The effect of perceiving new groups between the time of the figure's first perception and its final recognition as a figure that has occurred more than once.

(*c*) The focality of perception—the chance falling of the figure in the focal point of vision, or more or less outside of it.

Each one of these points can be easily made the subject of experimental investigation and the three following sections give the results obtained:

(a) The Method of Memorizing.

During the course of the experiments it occurred to me to ascertain how long it would take to memorize a group of five figures so that they could be accurately drawn. While acting as subject in these experiments I discovered that this was largely dependent upon the method of memorizing. At first sight it

would seem that in looking at a group of figures and attempting to get them in mind for future reproduction, one has to do with a process of memorizing by visual imagery. But one has but to attempt the task to discover that besides the visual image there is something else which is a powerful aid to memory. And this is a more or less complete mental analysis of the figures, an analysis which it is utterly unnecessary for the subject to put in words. What are the figure's more elemental parts? How are they related? Does it resemble anything in real life? Such factors as these are elements which, most will admit, do not belong to the visual sensation, as such, nor to its more or less perfect replica, the visual image. The attempt to picture an object so as to be able to see it clearly with the mind's eye, and if need be, draw it, is one mental process. The effort to analyze an object, to see what it is made of, what it resembles, its possible use, etc., is another mental process. And while the two may go hand in hand, they need not; and it is possible to memorize by either method.

Experience, however, would indicate that visualization without analysis is rare and difficult. However, these are two very distinct methods of memorizing; and while it is not possible to use either in an absolutely pure and unadulterated form, still it is possible to make either visualization or analysis the predominating feature in the method of memorizing. This was attempted in the following series of experiments. The method of experiment was very simple. A group of five figures was exposed for a constant time and the subject was called upon to memorize the group by one or the other method. At the end of the time the group was covered and the subject called upon to draw what he remembered. The drawings were then rated, an approximately perfect drawing being given a credit of 1. An imperfect drawing, but still recognizable as being intended for one of the figures exposed, was given a credit of 0.5. An utterly unrecognizable drawing was given a credit of 0.1. An omission was counted as zero. It is not always possible to assign a drawing with certainty to one of the three

gradings. However, it is generally fairly easy and is better than calling everything perfect or zero.

After a little experimenting it became evident that the method of analysis had a decided advantage over that of visualization. After this was noticed, there might be a subconscious tendency to favor the marking by analysis and thereby strengthen the evidence for the point maintained. But however much one might do this I am sure that no system of conscientious marking could turn the balance in favor of visualization. To offset any such tendency I was a little stricter in marking the results obtained in memory by analysis than those obtained by visualization. And I believe that the markings given for analysis are a trifle too low.

A much more serious difficulty is that which arises from the attempt to exclude all analysis of a figure when one is trying to get a visual image of it fixed in the mind. Associations crop up spontaneously, and one simply cannot exclude all analysis of the figure. The subjects were instructed that when they were attempting to memorize by visual imagery they were not to mind any involuntary associations or analyses of the figure that might spring up, but still not to make any great effort to suppress them. The result is that the two sets of experiments really represent memory by visualization without attempt at analysis or association, and memory by analysis and associations without any attempt to acquire a definite mental image. It is much easier to memorize by analysis to the exclusion of imagery than *vice versa*. Subjects often remarked that figures were remembered by some association rather than by imagery when they were attempting to memorize by visualization. But it seldom happened in attempting to memorize by analysis and association that the figure was recalled by its visual image suddenly appearing without any apparent associational connections. As a result of this the markings for memory by visualization are considerably higher than they would be if the method could have been used in absolute purity.

SUBJECT B.

Sept. 27, 1907.

Imagery.	Association.
0.8	4.0
0.4	1.7
0.4	2.1
1.2	2.1
0.3	2.0
1.6	2.1
6) 4.7	6)14.0
0.8	2.3

Oct. 11, 1907. Oct. 25, 1907.

Imagery.	Association.
2.1	2.6
2.1	2.2
0.4	3.2
3.0	1.2
2.2	2.2
1.5	1.5
3.0	1.7
1.3	3.5
1.1	2.6
2.0	
1.2	9)20.7
1.5	2.3
12)21.4	
1.8	

Oct. 4, 1908.

Imagery.	Association.
2.5	3.0
2.0	3.2
2.1	2.1
2.1	3.0
1.1	1.5
2.0	3.5
6)11.8	6)16.3
2.0	2.7

Oct. 31, 1907.

Imagery.	Association.
1.5	3.1
0.1	2.1
3.1	3.2
1.6	2.2
1.5	3.0
5) 7.8	5)13.6
1.6	2.7

SUBJECT R.

Sept. 26, 1907.

Imagery.	Association.
2.6	5.0
2.0	4.5
1.0	5.0
3.0	2.5
3.1	5.0
1.5	5)22.0
6)13.2	4.4
2.2	

Oct. 3, 1907.

Imagery.	Association.
3.0	5.0
3.0	1.0
1.1	3.0
4.0	4.1
2.2	2.0
2.9	4.4
2.7	3.0
7)18.9	4.0
2.7	8)26.5
	3.3

Oct. 10, 1907.

Imagery.	Association.
2.2	2.7
4.5	3.6
3.0	5.0
2.5	2.7
2.0	4.5
5.0	4.0
3.0	3.5
3.1	4.1
1.5	5.0
1.3	5.0
2.1	3.1
2.5	5.0
12)32.7	12)48.2
2.7	4.0

SUBJECT MO.

April 18, 1907.

Imagery.	Association.
2.0	4.0
2.0	5.0
2.0	4.0
2.0	4.0
2.0	4.0
5)10.0	5)21.0
2.0	4.2

April 25, 1907.

Imagery.	Association.
4.0	3.0
2.5	5.0
2.0	3.0
2.0	4.0
3.0	4.5
2.0	5.0
6)15.5	6)24.5
2.6	4.1

Oct. 8, 1907.		Nov. 7, 1907.	
Imagery.	Association.	Imagery.	Association.
2.0	4.5	3.0	4.0
2.0	5.0	2.0	4.0
1.0	4.0	3.0	5.0
4.0	3.0	3.0	3.0
3.0	4.0	2.0	4.0
4.0	5.0	3.0	3.0
3.0	4.0	3.0	3.0
3.0	5.0	2.0	5.0
2.0	4.0	3.0	4.0
3.0	4.0	3.0	4.0
10)27.0	10)42.5	10)27.0	10)39.0
2.7	4.2	2.7	3.9

The above tables show in every case a decided advantage in favor of memory by association and analysis, over memory by imagery. It would therefore seem as if the psychological factors in the analysis and association of a figure add greatly to the mind's power of retaining and reproducing it. An objection, however, was suggested by a friend who was not inclined to give up so readily the primary importance of mental imagery in the process of memory. Perhaps in memory by association, he held, there is brought into play the motor imagery which is inhibited by the attempt at visualization. So that in what is termed memory by association we have really memory by visual imagery plus motor imagery, and therefore this is naturally the more favored form. That such was not the case is evident not only from evidence to be given later, but from the record that was kept of the cue by which the subjects fixed the figures in mind in memorizing by association. Not once did they mention any feeling of movement, as of outlining or drawing the figure or its parts, but the associations were always such as connected the figure with some known object, or analyzed it into parts, or some kind of description was given which was of itself insufficient to express the subject's full concept of the figure, but stood as a symbol for his mental state in regard to it.

Below are a few random samples of such associations, divided into three classes:

(*a*) Associations which connect the figure with an object in real life.

(*b*) A description which indicates some kind of an analysis of the figure.

(*c*) A designation which really expresses the subject's inability to associate or analyze the figure in the given time, but which nevertheless serves as a symbol of the figure and aids in its recall.

A. Object.	B. Analysis.	C. Symbol.
Scroll of paper.	Circle.	Curlycue.
Melon.	Curved figure with dot.	
A kind of handle.	A kind of hexagon.	Funny figure.
Star.		Something upside down.
Tulip.	A kind of oblong.	
Heart.	A kind of square figure.	The well-known figure.
Swastika.	A half-circle.	The unassociated figure.
Diamond with handle.	A kind of scroll.	
Dumb-bell.	Curves.	
Two turnips.		
Necktie.	Dots.	
	Squares	
	Points.	
	Pentagon with dot.	
	Cut triangle.	
	Cut quadrangle.	
	Something long and narrow with points.	

On looking over this list of associations one might be inclined to say that this so-called memory by association and analysis is really nothing but a process of naming the figures and remembering the words used. That might be so if the catch-word used to designate the figure generally were sufficient to express it truly. But that is not the case. The figure is never completely described. And the subject's task is not to remember his description of the figure but to remember the figure so as to be able to draw it. The word or words used serve to fix and

crystallize the mental state that was experienced in perceiving the figure. They are not that mental state nor do they fully express it. Why? Because there is more in that mental state than is given in the word. And how do we know this? Because the subject draws more than his words express.

It is an important thing to ascertain the real value of motor imagery in memorizing such figures as were used in our experiments. It is not evident from inspection that it is either inferior or superior to "memory by association." Accordingly a method was devised by which the value of motor imagery could be tested. The subject was allowed to trace the figures with a pointer in one set of experiments, thereby giving him an opportunity for the development of motor imagery. This set of experiments was compared in each sitting with a "visual" and "association" set. The time allowed for each experiment was that which sufficed for the subject to trace five figures, which varied from 10 to 13 seconds, with the three subjects. Each subject, however, had for visualization and association the time that allowed him to trace comfortably the five figures. The order in which the experiments are printed is that of the experiment.

SUBJECT BR.

Aug. 24, 1908.

Visual.	Motor.	Association.
1.0	3.0	4.0
1.5	3.5	3.5
0.5	3.0	3.5
3.1	2.6	3.5
2.6	1.1	3.1
1.1	2.6	3.6
6) 9.8	6)15.8	6)21.2
1.6	2.6	3.5

Sept. 28, 1908.

Association.	Visual.	Motor.
3.5	2.0	2.5
3.0	1.6	3.0
5.0	2.0	1.1
3.5	1.5	1.0
2.0	2.1	3.0
2.0	1.1	1.0
3.6	2.0	3.0
7)22.6	7)12.3	7)14.6
3.2	1.7	2.1

Oct. 5, 1908.

Motor.	Association.	Visual.
1.6	3.5	3.5
3.0	4.0	1.2
2.6	4.0	1.0
2.0	3.5	2.5
2.5	3.0	2.5
2.0	3.1	2.1
6)13.7	6)21.1	6)12.8
2.3	3.5	2.1

Oct. 19, 1908.

Visual.	Association.	Motor.
3.0	2.5	2.5
1.0	2.6	1.5
2.0	4.5	3.5
2.0	3.0	1.0
2.5	4.0	1.0
2.0	3.6	1.0
6)12.5	6)20.2	6)10.5
2.1	3.4	1.7

Subject S.

Aug. 26, 1908

Visual	Motor	Association
2.0	4.0	5.0
4.0	1.5	4.0
1.0	2.1	5.0
2.1	2.0	4.0
1.0	2.5	4.0
2.5	1.5	1.0
6)12.6	6)13.6	6)23.0
2.1	2.3	3.8

Sept. 3, 1908

Association	Visual	Motor
4.0	2.0	3.0
3.5	3.0	4.0
4.5	1.0	1.5
4.5	4.0	1.5
3.5	4.0	2.0
5.0	1.5	3.0
2.6	4.0	2.5
7)27.6	7)19.5	7)17.5
3.9	2.8	2.5

Sept. 10, 1908

Motor	Association	Visual
4.1	2.1	3.1
2.0	4.0	2.0
4.0	4.0	1.0
2.0	3.0	1.5
1.0	4.0	3.0
3.0	4.0	2.0
1.0	3.1	3.1
7)17.1	7)24.3	7)15.7
2.4	3.5	2.2

Sept. 17, 1908.

Association.	Motor.	Visual.
4.0	3.5	2.0
2.0	6.0	3.0
4.0	1.6	3.5
5.0	2.0	2.0
5.0	3.0	2.0
5)20.0	5)16.1	5)12.5
4.0	3.2	2.5

Sept. 24, 1908.

Visual.	Motor.	Association
3.0	2.5	4.0
2.5	3.5	1.5
3.0	3.0	3.1
3.0	2.5	4.0
1.0	1.0	3.5
1.0	2.0	2.5
1.6	3.5	3.5
2.5	2.5	4.0
8)17.6	8)20.5	8)26.1
2.2	2.6	3.3

SUBJECT WR.

Aug. 27, 1908.

Association.	Visual.	Motor.
5.0	4.0	5.0
4.5	2.5	3.5
2.5	3.0	3.1
4.0	3.0	1.5
3.5	4.0	3.5
4.5	2.5	3.5
3.5	2.5	1.0
7)27.5	7)21.5	7)21.1
3.9	3.1	3.0

Sept. 3, 1908.

Visual.	Motor.	Association.
2.0	4.0	4.0
4.0	1.0	5.0
4.0	3.0	4.5
2.5	2.0	5.0
2.0	2.0	4.0
5)14.5	5)12.0	5)22.5
2.9	2.4	4.5

Sept. 10, 1908.

Motor.	Association	Visual.
4.5	2.5	4.1
4.5	4.5	3.0
4.0	5.0	3.0
4.0	4.1	4.1
2.5	4.0	1.0
5)19.5	5)20.1	5)15.2
3.9*	4.0	3.0

Sept. 17, 1908.

Association.	Motor	Visual.
5.0	3.0	3.0
4.0	4.0	3.5
3.5	0.5	2.5
5.0	3.0	2.5
2.5	3.0	2.0
5)20.0	5)13.5	5)13.5
4.0	2.7	2.7

* In this set seven figures were recalled by involuntary association. Were these excluded the average would be reduced to 2.5.

Oct. 1, 1908.

Visual.	Association.	Motor
4.0	4.5	4.0
2.5	5.0	4.0
3.0	5.0	2.5
2.5	4.0	4.0
1.5	4.0	3.0
5)13.5	5)22.5	5)17.5
2.7	4.5	3.5

SUBJECT BR.

Table of Averages.

Association.	Motor.	Visual
3.5	2.6	1.6
3.2	2.1	1.7
3.5	2.3	2.1
3.4	1.7	2.1
4)13.6	4) 8.7	4) 7.5
3.4	2.2	1.9

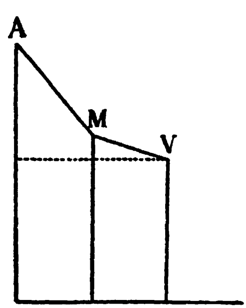


Figure 4, showing the relation between memory by association (A), motor (M), and visual (V) imagery for Subject Br.

SUBJECT S.

Table of Averages.

Association.	Motor.	Visual
3.8	2.3	2.1
3.9	2.5	2.8
3.5	2.4	2.2
4.0	3.2	2.5
3.3	2.6	2.2
5)18.5	5)13.0	5)11.8
3.7	2.6	2.3

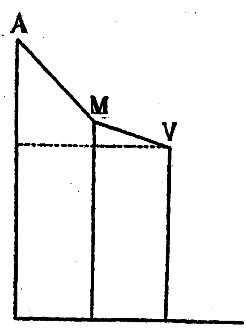


Figure 5, showing the relation between memory by association (A), motor (M), and visual (V) imagery for Subject S.

SUBJECT WR.

Table of Averages.

Association.	Visual.	Motor
3.9	3.1	3.0
4.5	2.9	2.4
4.0	3.1	3.9
4.0	2.7	2.7
4.5	2.7	3.5
5)20.9	5)14.5	5)15.5
4.2	2.9	3.1

Figure 6, showing the relation between memory by association (A), motor (M), and visual (V) imagery for Subject Wr.

These tables and figures show a decided advantage for memory by association over memory by imagery. They show besides that, so far as this advantage is concerned, it makes no difference whether the imagery be visual or motor. In motor-imagery we really have motor plus visual, for the subject necessarily looks at the figures while tracing them. So that even the combined effect of visual and motor imagery cannot equal the results obtained by analyzing and associating the figures.

(b) Memory as Related to the Sequence of the Surrounding Figures.

In the process of abstraction one group of sensations after another is perceived by the mind. In each one of these groups some common element is always contained. The moment arrives when this common element becomes separated from those that surround it, and approaches the focal point of consciousness. This is, as we have seen, not an instantaneous act but a process with more or less definite stages, and can under circumstances consume a relatively long time. In the meanwhile one group of impressions after another falls upon the mind. What, we may ask, is the effect of these impressions on the subject's memory of the common element? One might jump to the

conclusion that they tend to obliterate the memory of the common element. Still, one cannot be sure of this, off-hand; experiments have surprises in store.

Angell and Harwood found that distractions between a normal and compared stimulus not only did not always decrease the accuracy of recognition, but that at times they even increased it.[12] A very different result obtains where it is not a question of recognizing a stimulus but reproducing that which has previously been learned. Under these conditions the effect of sensory stimuli between the time of memorizing and of reproduction is to obliterate that which the subject had learned. Bigham found[14] the following average errors under these conditions:

	Empty interval	Optical filling	Acoustical filling
2 sec.	25.2	29.4	34.7
10 sec.	28.8	31.0	36.0
30 sec.	31.1	33.0	37.1

We might rest upon the results of Bigham and conclude that when a number of new figures are noticed after the perception of the common element they will tend to obliterate the memory of the common element. The lapse of time between the first and the second perception of the common element is not the only factor which tends to obliterate the common element. Succeeding impressions have a positive tendency to impair the subject's memory for the common element which has attracted his attention. However it seemed best not to rest content with experiments made under conditions that were not precisely the same as those under which we studied the process of abstraction. We put the point to an actual test

The method of experiment was as follows: The subject first saw three groups of figures He was instructed to fixate a given position in these groups *i.e.*, always the first, counting from

12 Angell and Harwood, "Discrimination of Clangs for Different Intervals of Time, Part I." *Amer. Journ. Psychol.*, 11, pp. 67-79; Part II, *ibid.*, 12, pp. 58-79.

13 J. Bigham, "Memory." *Psychol. Review*, I, 1894, p. 453.

left to right, or the second, or the third, etc. After these three groups came a fourth exposure. In this there was but one figure, which might be in any one of the five positions. After this came a certain number of groups—five, or twenty-one, or twenty-one blank spaces—according to the nature of the experiment. At the end of each experiment the subject's task was to draw the isolated figure of the fourth exposure.

The reason for varying the distance between the point of fixation and that of the exposure of the figure was to reproduce that condition of the previous experiments in which the subject is still unsatisfied with his knowledge about the common element. I first compared the condition of memory after five exposures with that after twenty-one. The subjects were rated as in the previous experiments on memory, and then an average of all experiments was taken. For memory after five exposures the general average for the subjects (twenty-six experiments) was 0.46. For memory after twenty-one exposures the general average for three subjects (thirty-one experiments) was 0.34. We thus see that increasing the number of exposures, after the perception of a figure, has a tendency to decrease the accuracy of the memory. I then compared the condition of memory after twenty-one groups of figures had followed the isolated figure with the condition when twenty-one blank spaces followed the isolated figure. The average in the first instance for the subjects (twelve experiments) was 0.67. In the second instance the average for three subjects (fifteen experiments) was 0.77. The perception of new figures seems therefore to have a tendency to obliterate the memory of the one already perceived. The element of practice, however, has almost doubled the markings, so too much reliance cannot be placed on these preparatory experiments.

It seemed possible to get results more quickly if we had a material in which each element was more homogeneous. One figure is so much more attractive than another and has so many more possibilities of association that the element of chance enters in to obscure the results. Consequently a set of numbers was

prepared. In each number there were three digits. In no number was the same digit repeated. A zero never occurred. An arithmetical sequence of the digits was avoided. Such numbers as the following were therefore excluded: 112, 120, 123. The order of experiments remained the same as before, only instead of figures we had numbers consisting of three digits each. A comparison was then made between the memory of an isolated number after twenty-one blank spaces had followed, with that after twenty-one groups of five numbers each had been exposed.

The memory was rated as either good or bad. If the subject recalled two or more digits correctly, his memory of the number was rated good; if he gave less than two digits correctly it was rated bad. The results are given below. The series with the vacant spaces are designated by V, the series with the twenty-one groups of figures are designated by "21." Δ indicates the difference in position between the point of fixation and the occurrence of the isolated element. Under *M* is given the rating of the subject's memory—good (g) or bad (b).

SUBJECT D.

V		"21"	
Δ	*M*	Δ	*M*
1	g	1	b
0	g	1	g
1	g	0	b
1	g	1	b
0	g	0	g
1	g	1	b
1	g	1	b
1	g	1	b
1	g	1	b
2	b	2	b
2	g	2	b
2	g	2	g
0	g	0	b
12g — 1b		3g	10b

SUBJECT MW.

V		"21"	
Δ	*M*	Δ	*M*
0	b	0	b
0	g	0	b
0	b	1	b
1	g	1	b
0	g	0	g
0	g	1	b
1	g	1	b
1	g	0	b
0	g		
7g — 2b		1g — 7b	

SUBJECT R.

V		"21"	
Δ	*M*	Δ	*M*
1	g	0	b
1	g	1	g
0	g	1	g
0	g	0	g
1	g	1	g
1	g	2	b
2	b	2	b
2	b	0	b
0	b	0	b
0	b	0	g
0	g		
7g — 4b		5g — 5b	

For all three subjects we have in the V Series 26g-7b and in the "21" Series 9g-22b. It is evident by inspection that memory is better in the V series. We may, however, express this better by using one of Pearson's auxiliary methods of correlation.[14] The one best adapted to the data at hand is

$$r = \sin \frac{\pi}{2} \frac{\sqrt{ad} - \sqrt{bc}}{\sqrt{ad} + \sqrt{bc}}$$

[14] Cf. Spearman, "The Proof and Measurement of Association between Two Things." *Amer. Journ. Psychol.*, 15, 1904, p. 82.

In this case a is the number of times the V series was good; b is the number of times it was bad; c is the number of times the "21" series was good; d the number of times it was bad. Accordingly we have

a	b	c	d
Vg	Vb	"21" g	"21" b
12	1	3	10
7	2	.1	7
7	4	5	5
26	7	9	22

$$r = \sin\frac{\pi}{2}\ \frac{\sqrt{(26).(22)}-\sqrt{(7).(9)}}{\sqrt{(26).(22)}+\sqrt{(7).(9)}} = 0.71$$

$$\text{Probable error} = \frac{1.1}{\sqrt{n}} = \frac{1.1}{\sqrt{33}} = \pm .19$$

$$r = 0.71 \pm .19$$

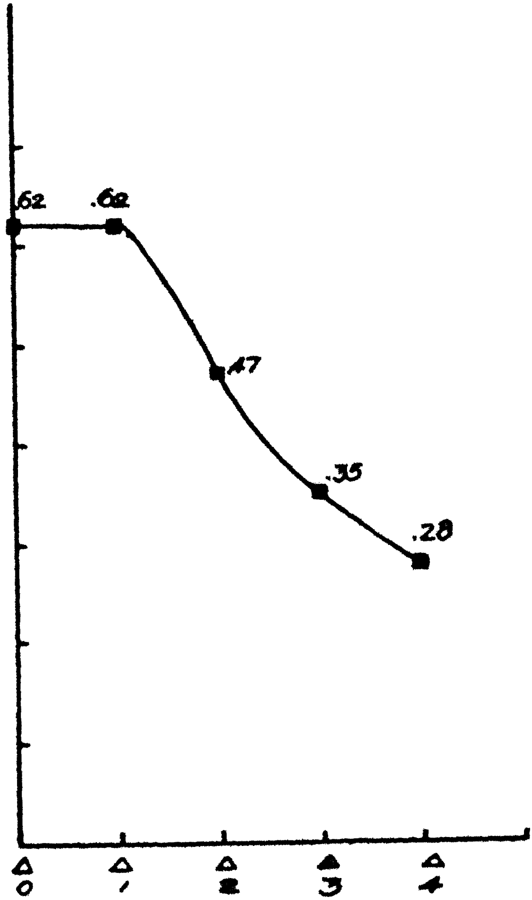
(c) Memory as Related to the Focality of Perception.

To give some idea of the decrease in the accuracy of the memory with the distance of the object from the point of fixation, I plotted a curve from the few preparatory experiments of the previous section. It happens to be of a smoothness that is not warranted by the few experiments and which I believe is somewhat accidental. In spite, however, of some objections that can be made to it, its main points represent fairly the decrease in the accuracy of the reproduction which is due to extra-focal perception.

A source of error lies in the fact that some figures have a greater attractiveness than others; they become more familiar to the subjects than the less favored figures. An extra-focal glance is sufficient for the perception of such figures, but not for that of unfamiliar figures. As a result the curve does not fall off as steeply as it should. However, it is not without value and is therefore given.

The abscissas in the accompanying cut give the difference between the point of fixation and the occurrence of the isolated

figure. The ordinates give the average ratings of memory for the corresponding experiments.



It is evident from the curve that the farther a figure is from the focal point of vision when it is perceived the less accurately it can be reproduced. When it is more than a single space away from the focal point the accuracy of reproduction commences suddenly to decrease.

4. The Process of Recognition.

(a) Analysis of the Experiments.

The experiments that are recorded in this section, though the last to be mentioned, were the first to be made. They are identical in time and nature with those recorded under the heading of Perception. The method is there fully described. It was found necessary to pick out and treat under separate sections that which our raw material offered to us concerning perception and recognition.

In the previous sections we have followed the process of abstraction from its initial stage—the breaking up of the group,—on through the process of perceiving and remembering the common element. We have picked out certain factors which favor and retard the memory of the common element and we come now to that mental process by which the common element is known to have been seen before when it is noticed again in the series. This process is that known as recognition. It is a process which is distinct and separate both in reason and in time from those that we have already described in our analysis of abstraction. The breaking up of the group—perception and memory—are more closely related to one another than to recognition. The breaking up of the group is really an initial stage of perception. Everything perceived is remembered more or less perfectly for a while. Indeed, the subject's memory of the figure is the result of the process of perception, for the experiments on memory by imagery and association have shown that memory is greatly dependent on the way in which the figure is perceived. But the recognition of the figure is a distinct and final stage in the process of abstraction. One need not make many experiments to prove that a figure may be perceived without being recognized. This is witnessed, if witness be necessary, by the subjects' remarking occasionally that early in the experiment they had noted the figure, which finally proved to be the common element. At that time, however, it did not occur to them that this figure was the common element.

There is indeed nothing remarkable in this. But it is somewhat strange that at the close of the experiment subjects would often say that they had seen the figure clearly, two, three or even four times before they stopped the apparatus. Three times seemed to be the usual number. Practice, and strict orders not to wait, did not stop the occurrence of such dilatoriness. This waiting may be due to two things:

(*a*) Time is required for the subject to resolve to stop the apparatus.

(*b*) It often takes an appreciable interval for the process of recognition to develop.

The first may be of importance; the latter certainly is. For in the course of the delay the subjects seem to have a dawning sense that the element in question is the common element. In fact it happened once that the series came to an end while the subject's mind was in this twilight state. He stopped the apparatus and then laughed and said: "Why, it just occurs to me that a figure I have in mind was undoubtedly the common element." There were several other occurrences of the same nature. It seems evident, therefore, that in abstraction there is a process of recognition distinct from the bare perception of the common element. Furthermore, this process of recognition may take an appreciable time for development.

We should expect to find in the process of abstraction a two-fold development, the development of perception and the development of recognition. As there are stages in the process of perception so there should be stages in the process of recognition.

What, then, is to be developed in the process of recognition? From the experiments of this section it is evident that recognition involves something that perception does not involve, namely, the element of certainty or uncertainty. Certainty that the figure has been seen before is what is dawning upon the subject in that state before his mind is fully made up.

If the process of perception were always completed before that of recognition began, our task would be a very simple one. We should have but to add to the stages of perception the various

degrees through which the subject goes in arriving at complete certainty, the different shades of probability up to unhesitating assent. But only by accident is the process of perception complete before recognition begins. What happens is that almost any degree of the certainty of recognition may coexist with any degree of the perfection of perception. And this we shall attempt to bring out in the arrangement of our results below. Three main points are picked out in the development of certainty:

The intimation of a common element. At the end of an experiment the subjects were asked to give a description of their mental state during the experiment. The first stage was a kind of inkling that a common element might be present. This stage I have designated as (1) an Intimation. It is a state of very weak probability and is sometimes due to the presence of similar figures. A more advanced stage I have designated as (2) Probability, and the final stage as (3) Certainty. These divisions of course are arbitrary and flow into each other.

With this explanation the various headings given below will be understood. The numerals in the parentheses show what figure was used as the common element in the experiment in question.[15] This will enable any one to see for himself to what extent the imperfection of perception was due to the complexity of the figure. In some cases at least the complexity of the figure must have been a minor factor. The final state of full recognition with certainty of the fact and a perfect idea of the form has not been put down in the following enumeration of the stages of recognition. It was the more common termination of the experiments and occurred with all subjects.

In order to give a more complete idea of the subject's mind his remarks have been occasionally transcribed and referred to by indices at the right of the numbers which designate the common element of the experiment. These remarks of the subjects, better than any description, give an idea of the course of development.

---

[15] Cf. Fig. 1, p. 118.

It required anywhere from about five to twenty-five exposures for the subject to find the common element. During all these exposures the process of abstraction was in a state of growth and development. Its stages were at first ascertained only by the subject's memory of what occurred during the experiment. Would it not be possible to get a direct observation of these stages by cutting the experiment short, thus giving fewer exposures than the subject ordinarily required? At the end of the experiment the subject would not have finished the process of abstraction; he would not have to recall as well as he could by memory the stages he had noticed in the development of complete certainty. He would simply have to describe by direct introspection his state of mind which would be, at the very moment, in some one of those stages of development.

Simple as the experiment seemed, it was most tantalizing to carry out. Sometimes the subject arrived at complete certainty before the shortened series came to an end. Sometimes the process of abstraction seemed not to have commenced at all, and the subject's mind was in a state of negative doubt.[16] By persistent effort, however, the various stages were confirmed, not all, however, with all subjects. The time necessary for this would have been excessive. However, a sufficient number of confirmations of the previous results of memory were obtained to make it evident that the stages of development so obtained were no delusions but states of mind that actually occurred. These confirmatory experiments are noted under the same division as the previous ones wherever they were obtained. The words "confirmed by" precede the numbers that refer to the common element of the shortened series.

---

[16] By negative doubt I mean a state of mind in which the subject knows of no evidence either for or against a proposition. In this case the proposition would be: A common element is present on this disk. Positive doubt would occur if one were moved equally by the known evidence for and that against a given proposition.

The Stages of Recognition.

SUBJECT BE.

I. An intimation of a common element, without any knowledge of its form.

(4, 14) (14, 11) (7, 10) (9, 10) (15, 11) (9, 13) (17, 14) (13, 11) (14, 11).

II. Probability that a common element is present, but an imperfect idea of the form.

(3, 11)* (17, 11).

Confirmed by (16, 12) (5, 9) (4, 9).

* Subject knew at first that a curved figure was presented.

III. Probability that a common element is present, and a true idea of its form.

Confirmed by (15, 16) (16, 16).

IV. Certainty that a common element is present, but an imperfect idea of its form.

(10, 14)* (15, 12)† (2, 14) (15, 13) (11, 13) (4, 11)‡ (15, 13) (8, 13) (9, 11).

* Subject could not draw figure at all, but was certain that a common element was present.

† Subject was certain that the figure appeared several times, and that it was not one of the ordinary geometrical figures.

‡ Forgot the image of the figure, but was able to find it in the table of figures.

Reaction of the subject to disks with no common element:

(1) Not certain of any common element. But he drew a figure which might perhaps have been common. It was an amalgamation of several similar figures which had occurred on the disk.

(2) Negative doubt.

(3) Negative doubt.

(4) Negative doubt.

(5) Negative doubt.

(6) Inclination to believe that no common element was present.

(7) Very uncertain. If forced to guess one way or the other would say that a common element was present.

(8) More probably no common element was present.

(9) Probably a common element was present. Subject drew as the common element a figure which was a combination of two similar figures that occurred on three disks.

(10) Perhaps a common element was present

SUBJECT BL.

I. An intimation of a common element, without any knowledge of its form.

(7, 10)-(15, 11)-(17, 13)*-(17, 7).

(Confirmed in two cases, but neglected to note what figure was used.)

* Subject said he was quite certain that for a time he knew a common element was present. What this common element was remained subconscious.

II. Probability that a common element is present, but an imperfect idea of its form.

(4, 14)*-(10, 7)†-(17, 8)-(15, 10).

Confirmed by (5, 9).

* Thought for a time that a dark spot would turn out to be the common element, as indeed it actually did.

† The subject was at first conscious of a cloudy flake, which afterwards cleared up.

III. Certainty that a common element is present, but an imperfect idea of its form.

(5, 12)*-(16, 6)†-(14, 10)-(8, 10)-(16, 6)-(10, 7)-(9, 11).

Confirmed by (7, 12).

* The subject felt conscious of a difference in the size of the figures, and classified them into large and small. At a certain period in the experiment he knew that the common element was not one of the larger figures.

† The subject knew for a time that the common element was some kind of an unpleasant unsymmetrical figure.

Reaction of the subject to disks with no common element:

(1) Subject stopped the apparatus after twenty four expositions and said that he noticed no easing of his task as the experiment proceeded. When a common element is present he notices that the process of perception becomes easier as the experiment proceeds.

(2) Nothing noted.

(3) Nothing noted.

(4) Not certain that no common element was present.

(5) Not quite certain that no common element was present.

(6) Almost certain that no common element was present.

(7) More probably no common element present.

(8) Absolute indecision.

(9) Indeterminate.

SUBJECT D.

I. An intimation of a common element, without any knowledge of its form.

This stage was not found with this subject.

II. Probability that a common element is present, but an imperfect idea of its form.

(15, 10)·(10, 10)*·(8, 16)·(4, 13)·(3, 9)†·(4, 9)‡·(16, 8).

* Wondered for a time whether or not a blurred figure seen out of focal vision was going to turn out to be the common element.

† Subject could describe the figure as symmetrical and oval at one end and said she had a visual image of it; but knew that this visual image was incorrect.

‡ Probably a funny little figure round at one end, with one line coming to a point.

III. Certainty that a common element is present, but an imperfect idea of its form.

(10, 12)·(10, 16)·(15, 12)·(15, 9).

Reaction of the subject to disks with no common element:

(1) Not sure that no common element was present.

(2) Possibly a tall, slender figure was the common element.

(3) Slight probability of a common element.

(4) Thinks no common element was present.

Subject Kr.

I. An intimation of a common element, without any knowledge of its form.

(13, 16)*·(16, 16)·(15, 11)·(15, 10)·(13, 11)·(17, 15)†·(6, 12)·(10, 13)·(4, 13)·(17, 10)‡·(17, 13)‖·(14, 13).

Confirmed by (7, 13).

* At end of experiment subject drew figure and said he was perfectly certain that it was the common element. Long before he arrived at certainty he had a vague *feeling* that some kind of a common element was present. Later he knew that it was changing position.

† "Zuerst ein unbestimmtes Gefühl, ohne Object, sehr vag."

‡ At first a general probability.

‖ "Increasing feeling of probability, without contents," was an early stage in this experiment.

II. Probability that a common element is present, but an imperfect idea of its form.

(15, 11)*·(4, 13)†·(13, 11).

Confirmed by (16, 13).

* The subject had at first a feeling that probably a curved figure was the common element.

† A feeling that the common element might be a figure limited by straight lines.

III. Probability that a common element is present, and a true idea of its form.

This stage was found with subject Kr in the series of confirmation experiments with (16, 14).*

* The image of the figure came to him only after the experiment was over.

IV. Certainty that a common element is present, but an imperfect idea of its form.

(8, 13)-(13, 12)-(16, 6)-(15, 12)-(17, 11).

Reaction of the subject to disks with no common element:

(1) Uncertainty.

(2) Complete uncertainty.

(3) Thinks no common element present.

(4) In first part of experiment a very weak feeling of probability that a common element was present. During about three-fourths of the experiment, an ever increasing probability that no common element was present.

(5) Certainty after 23 expositions that no common element was present.

(6) Very probably no common element.

SUBJECT Mo.

I. An intimation of a common element, without any knowledge of its form.

(8, 12)-(11, 12)-(13, 12)-(6, 12)-(3, 13)-(8, 10)*-(12, 14)-(10, 12).

* There was a first stage of vague probability followed by a blank, and then a rising probability accompanied at first by no definite image.

II. Probability that a common element is present, but an imperfect idea of its form.

(14, 10)*-(14, 15)†-(12, 14)-(10, 12)-(7, 12).‡

* "There was a time when I thought to myself, 'It is a pointed figure.'"

† "At first I thought an open kind of figure would be present."

‡ There was a time when a strong probability attached itself to some kind of a figure with an angle cut out.

III. Probability that a common element is present, and a true idea of its form.

(12, 13)-(2, 16)*-(4, 12)-(14, 11)-(1, 13).

* "There was a clearly marked period of doubt during which I thought that a triangular figure was being repeated."

IV. Certainty that a common element is present, but an imperfect idea of its form.

(4, 9).*

* "For a part of this experiment I knew a figure was being repeated, but I could not catch it. I had very little or no idea of its form."

Reaction of the subject to disks with no common element:

(1) A weak probability that some kind of a common element is present.

(2) A faint probability of a common element with cut-out angles. (A confusion of several figures.)

(3) Faint probability of a common element.

(4) Saw nothing.

(5) Very slight probability of a common element.

(6) Sure that no common element was present.

(7) Very weak probability of a common element.

(8) Negative doubt.

(9) Negative doubt.

(10) Probably no common element present.

(11) Probably no common element present.

Subject R.

I. An intimation of a common element present, without any knowledge of its form.

This stage was not to be found with this subject.

II. Probability that a common element is present, but an imperfect idea of its form.

(12, 14)* (2, 6)† (13, 11) (13, 15).

* The idea of something like a heart came first; then the thought that it was different from a heart.

† Noticed at first the outer points.

III. Probability that a common element is present, and a true idea of its form.

(9, 10).

IV. Certainty that a common element is present, but an imperfect idea of its form.

(13, 12)* (14, 11) (4, 14) (2, 6) (2, 13).†

* Subject stopped the apparatus before she was sure of the form. There was a time in the experiment when the subject knew that some figure was being repeated, but did not know just what one it was.

† Saw figure three times. On second exposition she knew that the figure had points and that was all.

Reaction of the subject to disks with no common element:

(1) Nothing noted.

(2) No intimation of a common element.

(3) Probably a common element repeated.

(4) Negative doubt.

(5) Thinks no common element present.

(6) Almost sure that no common element was present.

(7) Thinks no common element present.

Subject W.

I. An intimation of a common element, without any knowledge of its form.

(2, 13)-(13, 3)-(4, 12)*-(17, 13)-(17, 12)†-(17, 13).‡

Confirmed by (12, 15)-(2, 6).

* Subject saw something changing its position before he could make out its shape.

† Subject was conscious of a common element all along, but did not know what it was.

‡ Subject said he was conscious of a common element before there "really was one present."

II. Probability that a common element is present, but an imperfect idea of its form.

(8, 14)-(8, 12).

Confirmed by (15, 12).*

* Subject thinks that a common element of angular form was probably the repeated figure.

III. Probability that a common element is present, and a true idea of its form.

(4, 13).

IV. Certainty that a common element is present, but an imperfect idea of its form.

(6, 10)-(12, 14)-(3, 6)-(1, 13).

Confirmed by (14, 13)*-(3, 9).†

* Subject noticed that the image faded away very rapidly.

† Subject saw a rounded thing suggesting two points and knew that it was repeated. At end of experiment (10th exposure) he could not draw it. He had a "feeling that it was there before he saw it."

Reaction of the subject to disks with no common element:

(1) Sees nothing.

(2) Thinks none absolutely alike.

(3) Negative doubt.

Subject Z.

I. An intimation of a common element, without any knowledge of its form.

(8, 12)*-(13, 11)-(8, 13)-(4, 14)†-(2, 6)‡-(16, 6)‡-(3, 6)-(2, 13).‖

Confirmed by (15, 14) in two different experiments.

* At first it seemed to the subject as if a common element was present. Then he looked here and there to find it.

† At first there was an abstract feeling of something common.

‡ First noted something common and new.

‖ At first there was an indefinite consciousness of something repeating itself.

II. Certainty that a common element is present, without any knowledge of its form.

(3, 11).

III. Probability that a common element is present, but an imperfect idea of its form.

This stage is not to be found in the records of this subject.

IV. Probability that a common element is present, and a true idea of its form.

(2, 13) (5, 12) (10, 10).

Confirmed by (16, 13) (10, 12).

V. Certainty that a figure is being repeated, but an imperfect idea of its form.

(7, 12) (13, 11)* (17, 14).

* Subject knew that the common element had something round in the middle.

The following observations of this subject are interesting:

(*a*) "There is no time to compare one figure with another, or one impression with a previous impression."

(*b*) When a figure was used as a common element which the subject had not seen before, it generally happened that at first he noticed something new and then a special figure.

(*c*) The perception of the common element has a tendency to obliterate the images of the other figures. Before perceiving the common element as common, the images of several figures that have just passed by float about in the mind. When the common element is perceived as such, they vanish at once.

Reaction of the subject to disks with no common element:

(1) Stops apparatus after nine exposures, and says he is perfectly certain that no figure is repeated in each group.

(2) After nine exposures the subject was certain that no common element was present.

(3) Subject thinks that a figure (4, 14) might possibly have been repeated. He drew it correctly along with another figure; which two figures were drawn when he was requested to reproduce everything he could remember as having been seen. This also happened with (17, 15).

(4) No intimation of any figure having been repeated.

(5) No intimation of any figure having been repeated.

(6) Thinks that there was no common element.

(7) State of negative doubt.

(8) Almost certain that no common element was present.

(9) Negative doubt.

(10) Thinks that no common element was present.

(11) Subject thought several times that a common element was present. Then there came an ever increasing certainty that none was present, and at the end of the series he was certain that there was no common element.

Another interesting stage with this subject is that in which the figure on being first noticed is recognized as familiar. By the word familiar, it is not meant that he had seen it before on other disks but that it comes into focal consciousness with a peculiar nuance which tells the subject that this is the common element. It seems that this tone of familiarity (*Bekanntheits-qualität*) arises from the figure's being seen before but not analyzed out from the other figures.

The subject whose results are about to be recounted could give by introspection at the end of the experiment no information at all about the development of the mental process he had just experienced. When later on in the semester I commenced to confirm the results of self-observation, I tried the same method with this subject. I cut the experiment short after he had seen fifteen groups of figures and then asked him simply: "What do you think? Is there a common element present or not? Are you certain or merely inclined more or less to think that you see a common element? Draw what you remember!" In this way was obtained what the subject's introspective memory failed to reveal. Cross-sections were obtained in the course of development and fixed before they could fade from memory.

SUBJECT U.

I. An intimation of a common element, without any knowledge of its form.

(5, 12)-(7, 13)-(10, 9)-(10, 12).

II. Probability that a common element is present, but an imperfect idea of its form.

(2, 6)-(17, 15)-(16, 13)-(4, 9)-(2, 12).

III. Probability that a common element is present, and a true idea of its form.

This stage was not found with this subject.

IV. Certainty that a common element is present, but an imperfect idea of its form.

(10, 7)·(4, 14)·(8, 10)·(10, 6)* (5, 9).

* Certain only that he had seen the "two eyes" recur.

Reaction of the subject to disks with no common element:

(1) No idea of any common element at the end of the experiment.

(2) No idea of any common element at the end of the experiment.

(3) Complete uncertainty at end of experiment.

(4) No idea of any common element at the end of the experiment.

(5) No common element noted.

(b) Interpretation of the Results.

(i) The Immediate Experimental Conclusions.

When we look at these results it becomes at once apparent that an element of certainty and uncertainty is involved in the process of recognition. If we ask ourselves what this means we must say that whenever the mind is certain of anything, it assents; and whenever we have an assent we have an act of judgment. One of the immediate empirical conclusions of our results may be stated thus: *The process of recognition involves an element of certainty or uncertainty.*

From this we may conclude: *That the process of recognition involves a judgment or a suspended judgment.* For whenever I am certain I assent; and whenever my mind is in a state of uncertainty, assent is suspended. In the one case there is a judgment; in the other, judgment is suspended. It is not necessary that this judgment should be formulated in so many words. In fact, one may venture to say that in most cases of perfect recognition there is no verbal formulation of the judgment at all; but the psychological act of judging is nevertheless really and truly present.

The presence of a judgment in the act of recognition proves that the act of perception which does not involve a judgment is

an essentially different and less highly developed mental state. Recognition is indeed a perception, and over and above this a judgment is passed upon the perception. This judgment involves the statement that what is now perceived has been perceived before. If recognition is incomplete the judgment hangs in suspense and cannot be definitely passed.

In the further study of recognition we have only to ask ourselves, what is the basis of this judgment? Do the experiments help us out?

If we run through the results we will find that any degree of certainty may be accompanied by any degree of the perfection of perception. A person can be certain that a figure was repeated and have a perfect image of the figure, or an imperfect image, or no image at all. A second empirical conclusion may be stated thus: *Assured recognition is not dependent upon perfect perception.* And why this statement? Simply because it is an empirical fact that assured recognition can exist with a very imperfect perception, a perception that is so imperfect that it involves no mental image whatsoever.[17]

While indeed we have not found out, as yet, on what the judgment of recognition depends, we have at least discovered something on which it does not depend. And that is the mental image. This suffices finally to dispose of one theory of recognition, now generally rejected by psychologists—the theory, namely, that recognition is brought about by the comparison of the present sensation with a revived mental image. Identity being perceived, the object seen is then recognized. That such a comparison of images is unnecessary appears from the experiments. Why? Because recognition takes place not only when there is no revived mental image of the past perception, but when the present perception itself is too imperfect to leave any trace of mental imagery in the mind. Recognition, however, may take place by a comparison of mental images. In general the rapidity of succeeding impressions made this an impossible, or at least a very awkward, process. It once hap-

[17] Cf. also above, pp. 134-136.

pened, however, that a subject reported that she had used just this method in arriving at certainty of recognition. On thinking that she had seen a certain figure twice, she tried to call up the previous image that she had in mind as identical with the figure just seen, and institute a comparison between the two images. However we must note that *recognition was already in the probable stage* when this was done. And the *comparison* that was attempted was after all only *an auxiliary method.*

The comparison of images, therefore, may come in as an aid, but it is not necessary to recognition nor is it the normal method. One might object to the use of the word 'normal' here as carrying us beyond the limits of legitimate deduction. Was not the rapidity with which the exposures succeeded one another expressly chosen to exclude the possibility of comparing mental images? That is true, and our experiments prove only that the comparison of mental images is not necessary in the process of recognition. As to its being the normal method, we can from our own experiments only conjecture. But there are other experiments along this line. I refer to those on the recognition of the identity of time intervals, tones, etc. When a subject listens to two raps separated by a short interval, and then, after a period of waiting, hears two more raps, how is it that he recognizes that the second two raps mark off an interval of time equal to that of the first? Does he really compare some kind of mental images of the two time intervals? It would seem from the experimental research on this point, that he does not.[18]

Professor Frank Angell has made it abundantly clear that the recognition of tones does not depend on a comparison of mental images. In his study of the "Discrimination of Clangs for Different Intervals of Time," he arrived at the following results:

"The main conclusion to be drawn from the distraction experiments is that judgments of tone discriminations can take place, and in the majority of our experiments did take place,

[18] Cf. Wundt, *Physiologische Psychologie,* III, 5, 476-517.

without conscious comparison between the present sensation and a memory-image of a past sensation. When, for example, a reagent, after a long time-interval filled with interesting reading, from which he had to be practically aroused by a sharp signal in order to prepare himself for the apprehension of the second tone, nevertheless delivered a judgment with a feeling of considerable security, it is idle to speak of "memory-images" or indeed of comparison in the ordinary meaning of the word. Or when a reagent, after having accurately discriminated six pairs of tones, decided with ease that a tone just given is like or unlike a tone 4 vibrations higher or lower sounded 60 seconds before, and is correct in these decisions 63 times in 100, it is evident that the ordinary theories of tone-comparisons need readjustment.

"No more is it explicable on the theory of memory comparison that there should not have been a great increase in doubtful judgments in passing from undistracted to distracted discrimination, or indeed in failures to judge at all, or that the several forms of distraction should not have shown a far greater difference in effect than was actually the case."[19]

In the light, then, of our experiments, and also those on the recognition of various sensory stimuli, it is not too much to say that the comparison of mental images is not the normal method of recognition.

Summing up, then, the conclusions that we may regard as established by the experiments of this section we may state:

A. The process of recognition involves an element of certainty or uncertainty. From this follows:

The process of recognition involves a judgment or a suspended judgment.

B. Certain recognition is not dependent on perfect perception. From this it was seen to follow that:

A comparison of mental images is not necessary to the process of recognition.

---

[19] *Amer. Journ. Psychol.*, 12, 1900-1901, p. 69.

An empirical fact rather than a conclusion from these experiments is stated in the following proposition:

Certain recognition can take place without the formation of any mental image of the thing that is recognized.[20]

(ii) The Basis of Judgment in Recognition.

When we are asked to give an account of the real basis of judgment in recognition we naturally ask, how is the object that is recognized remembered? The factors of memory, one might suppose, are active to a large extent in the process of recognition. We are naturally concerned with the factors which enabled the subjects to memorize the figures used in our experiments which represented rather complex conditions. It cannot be taken for granted that the basis of recognition is the same for simple sensations and complex perceptions. In fact it is rather likely that what serves as our cue in one case does not meet the demands of another, that what is the chief basis of recognition of a simple tone may become a very minor factor in the recognition of a time-interval. And what is prominent in recognizing a time-interval may become subordinate in the recognition of a street or a house as places where one has been before. On this account it is desirable to take for experiment such complex material as our figures, in order to see if any factors enter into the process of recognition that have not been noticed in the usual experiments on time-intervals, colors, tones, etc.

The process by which the figures are remembered should give us some clue to the method used in their recognition. In the section entitled, "The Factor of Memory in the Process of Abstraction" (p. 139), we compared memory by visualization and by motor imagery, with memory by association and analysis. A marked advantage was found in favor of the latter. Memory by association consisted in relating the figure to known objects, or analyzing it and thus relating it to certain mental categories. These mental categories are the

20 This conclusion is based in great part upon the experiments given on pp. 134 ff.

bonds which hold the figure in place and make possible its recall when it has left the field of consciousness. In fact, it seems that if all the conceptual ties could be cut, or be lacking from the beginning, the figure would fade away completely and recall would be impossible. We find also that, in the process of perception, the essential element is not the formation of a visual image but the relating of the object perceived to one or more mental categories. Nor must we regard this relating of the impression of an object to its categories as a manipulation of separate and distinct psychical entities. It is rather what Wundt would call an assimilation. The sensation and the general concepts form a psychical compound which differs from its elements and is a new mental product. What are the elements of this compound? Wundt speaks of the feelings involved, especially that of familiarity, the sensation and the images to which it is assimilated. But we may question the completeness of this analysis.

There seems to be something that is not included therein and that something lies in the mental categories that couple the perception of the object to the train of memory. These, the essential elements of assimilation in perception, are also the elements *par excellence* of recognition. An assimilation does take place, and on the basis of our experiments on memory and the analysis of perception we may venture to say that the chief elements of assimilation are the concepts to which the sensations are assimilated in the process of perception. When the figure is seen it is at once assimilated to certain mental categories; it is regarded as made of straight or curved lines; it has elements that curve; it is an open or a dark figure; it is symmetrical and regular, or just the reverse. These phrases do not stand for images that are present; this the cases of recognition without imagery prove. But suppose they do so stand; suppose we have in recognition an assimilation of a present sensation to a number of revived images—of lines, curves, points, etc. Certainly the new psychical product should be an image, a product of the sensation and the imagery of past experience. But there were

cases in which recognition took place without the trace of an image. Consequently the assimilation would appear to be of elements that are not images. These elements we may speak of as mental categories or concepts. The sensation of a figure never stands alone. Perhaps no sensation ever does. It is related to an appropriate series of concepts. These are not all in focal consciousness. Perhaps all remain unanalyzed in the background of consciousness until by reflection we consider what kind of a figure we have seen. But the sensation plus the concepts with which it is associated,—these are assimilated and constitute a new psychical product. This psychical product is what is known as our 'idea' of the figure. My subjects have sometimes said: "I have an idea of what the figure is, but I cannot draw it." And then after some thought they would give a very inadequate description which would relate the figure to some concept. On being allowed to look for the figure they would find it among the entire lot of figures that made up our material. Our 'idea' of the figure is whatever image may be present plus the concepts to which it is assimilated. *That which is the chief factor in perception, that by which we recall figures, is also that by which we recognize them.* And this is the figure's series of associated concepts. When a figure is seen once, some kind of an 'idea' of the figure is formed—it is fitted in to one or more mental categories. When it is seen again the new percept is assimilated to the old. The old series of associated concepts falls in with the new. And in this way, perhaps, is produced the tone of familiarity. In the process of assimilation there is nothing that jars; on the contrary there is a reinforcement at least of some members of the associated train of concepts. New concepts may be brought out, but they fit in with the old. Merely similar figures, however, might on a later perception bring out new concepts which would contradict the old and thereby destroy the feeling of familiarity and give rise to doubt as to the identity of the figures.

One who is not disposed to give such individuality to the concept as distinct from sensation and mental imagery might

have recourse, as Wundt does, to the feelings. When we find no image in the process of recognition we must not jump to a conclusion that a concept distinct from our mental imagery is present. There are the feelings to be taken into consideration. Perhaps these mental categories are groups of feelings and not a class of mental states by themselves. The examination of this point leads us to our next chapter, in which we analyze the product of abstraction.

IV.

THE PRODUCT OF THE PROCESS OF ABSTRACTION.

In our analysis of the process by which an abstraction is formed, we have necessarily learned something about the final product. We have watched the growth of a complex mental state and must necessarily know something about that mental state in its final stage. Are there any evident elements in the final product of abstraction that we may regard as facts of experience? Yes. Our experiments have revealed some to which we called attention in our section on the process of perception. From the results of that section, confirmed as they are by the succeeding chapters, there are two important facts that were abundantly evident.

(*a*) *There exist imageless mental contents representative of a visible object.* Our own experiments are not the only evidence on this point. A reference to the history of the problem[1] will show that a number of psychologists have determined the existence of various kinds of imageless mental contents. The consensus of evidence is such that 'thoughts' without imagery must be looked upon as established mental facts. And when we take perception to mean the result of the process of perception, our experiments show conclusively that we can have a perception of a visible object in which there is no visual imagery. Our idea of that visual object is therefore not a mental picture, although under such conditions as obtained in our experiments we should expect, if at all, to find visual imagery constantly developed.

Without, however, making any assumption as to the nature of these imageless mental contents we may regard their existence as an established fact. They are the essential elements in the product of perception and abstraction. The existence of any

[1] Cf. pp. 76 ff.

kind of mental imagery in the complex product is not essential. Imageless mental content and not imagery is therefore the true product of abstraction.

The second fact of experiment is this: (*b*) *Perception is a process of assimilating the data of sense experience to their appropriate mental categories.* By this assimilation the object is perceived. The word category is not here taken in any preconceived sense. It is a fact that in perceiving a figure the earlier stages were designated as a knowledge that the figure was "pointed" or "open" or "round" or "had the top lines crossed," etc. These expressions are examples of what I mean here by categories. It is a fact, too, that these expressions were not descriptions of mental images. The figures, however, had been seen with the eyes, and in perceiving them they were interpreted in terms of the previous knowledge of the subject. This I have expressed by saying that the figures were assimilated to appropriate mental categories. So far this is all that I mean by the word category.

Let us now ask, what are these mental categories in terms of our modern psychological terminology? A current psychological division of our mental states leaves room for nothing but (*a*) sensations and their images, (*b*) feelings, and (*c*) will, which by some psychologists is explained in terms of feeling. To these states and combinations of them many psychologists have attempted to reduce our mental processes. We may now ask ourselves to which of these classes do the mental categories of perception belong?

(1) Do they belong to the class of sensations and images?

The 'mental categories' are not, of course, sensations, and we have already shown that they can not be directly interpreted as images, because they exist without imagery. Dr. Ach, however, has a theory[2] by which they might be the combined effect of many images. They are not images but the tendencies of a whole host of images to reproduce themselves. This theory was excogitated to explain the meaning of words. A word is

[2] Cf. above, p. 86.

understood because it sets a number of images in readiness, all of which have a tendency to reproduce themselves. This tendency of the images to reproduce themselves is the meaning of the word.

Against this as a theory of the meaning of words one may object:

(*a*) If a single image can not constitute a meaning it is hard to see how the tendency of a whole host of meaningless images to come into consciousness would constitute a meaning.

(*b*) If we refer to the section in the experiments of Bühler[3] entitled "Ueber das Auffassen von Gedanken" we will see that the 'mental categories' which were used by his subjects in the understanding of sentences cannot be analyzed into any known form of mental imagery.

(*c*) Furthermore, words express objects for which we can have no adequate imagery. How then can the mere tendency of this inadequate imagery to reproduce itself constitute the meaning of the word?

The same objections which prevent our acceptance of Ach's theory as an explanation of the meaning of words preclude its application to the 'mental categories' of our own experiments.

It would explain meaning by the tendency of meaningless mental contents to reproduce themselves; for pure sensation independent of its associations has no meaning; neither has an image. It must be associated with other mental states to be understood. If these mental states are themselves but a host of images, each one of which has no significance in itself, from their combinations we can not bring about meaning. Nor can this tendency to appear in consciousness be said to constitute meaning. For the mere tendency of meaningless mental states to appear in consciousness would give no meaning that was not in these states themselves.

One might challenge the statement that pure sensations or mental images independent of their associations have no meaning. Let us therefore develop this point a little further.

[3] *Archiv für die ges. Psychol.*, 12, pp. 12 ff.

Whatever may be our theory, it is a fact that a complex of sensations on being received into the mind *is* interpreted. This is evident from our section on perception. The interpretation takes place by means of the something that we may term 'mental categories,' to which the sensation is associated. These give it a meaning. But suppose the sensation is not assimilated to these mental categories? Is this not merely to say that it is not understood and has no meaning? What is left to meaning when you deprive it of every possible association and every mental category into which it might be resolved? It dwindles to nothing and ceases to be meaning.

These mental categories possess meaning by their own right and are qualitatively distinct from sensations and images.

One might bring in at this point Ribot's 'intentional' theory of the mental image.[4] Sensations and mental images are signs of their objects. But as we said in our passing criticism of Ribot, if the mental image is a sign of the object that it represents, it must be understood. On one side of the sign is the object signified, on the other is the meaning of the sign. If the mental image is a sign it must not only have an object but also a meaning. Consequently, to say that the image is a sign does not help us to get along without any kind of an idea or concept which functions as a meaning. If, therefore, by acting as a sign sensations and mental images cannot account for meaning, if they themselves are not the meaning, we must seek for meaning elsewhere than in sensations and their mental images.

However, if we could take Dr. Ach's *"Vorstellung"* in the sense of a mental 'concept with meaning' we have in the theory a good analysis of a number of those states which Marbe and his followers have termed *"Bewusstseinslagen."*[5] They are mental states in which several concepts tend to appear in consciousness—but no one succeeds in doing so. As a result, you have a more or less unanalyzable mental state without definite

[4] Cf. above, pp. 78 ff.

[5] Cf. above, p. 85.

characteristic. The tendency of the many 'concepts with meaning' to appear in consciousness results in an imageless mental content, which is hard to characterize, simply because many characteristics tend to come before the mind but no one succeeds in doing so.

(2) Are the 'mental categories' feelings?

Those who hold to the opinion that feelings of pleasure and pain constitute the sole elements of our emotional life will not be disposed to seek in these affective states an interpretation of our 'mental categories.' These 'mental categories' express knowledge; and knowledge is not pleasure and pain, though it may be pleasurable or painful. Nor does it make any difference how we may extend the idea of feeling; if we still mean by it something that is not knowledge, then thoughts and 'mental categories' can never be explained in terms of feeling. For if the word 'feelings' remains an exact scientific term to designate those very mental processes which do not give us knowledge, if feeling is opposed to sensation and to all our cognitive mental processes, then the 'mental categories' we have defined above are not states of feeling.

Such considerations as these could hardly have escaped Wundt. Yet he would interpret our 'thoughts,' and I suppose what I have termed 'mental categories,' as a complex of images and the "adequate" feelings which are involved. Our 'mental categories,' he claims, are not feelings alone and not images alone, but a complex of both. But if imagery is in itself meaningless, if we can have 'thoughts' which are not images, then the representative function of our thoughts and 'mental categories' must be performed by the 'feelings.' No single individual can place a limit to the meaning of a term. Thorndike calls every single one of our mental processes a 'feeling.' To this even Wundt would object. Still, if he were to insist on embracing under the term 'feelings' the representative content of our thoughts as well as their affective tone, he should at least admit that there are two very distinct classes of feeling —one which gives the affective tone and another which repre-

sents the object. Wundt has nowhere made this admission. In fact, from his writings it would seem that the representative function is ascribed by him to the imagery in the complex mental content termed a 'thought.' But on being accused of this by Bühler he strenuously objected that Bühler had not read his works[6] and maintained that in his analysis of thought there was also the concept of the feelings. Consequently, the question arises: Do these feelings represent the object or not? If not, they can never account for the representative function of 'thought.' If they do, then surely we must classify our feelings into those that represent an object and those that do not; for it is certainly clear that there exists a large class of feelings which are not representative of objects.

If then there are 'feelings' which can represent an object, how, we may ask, does this come about? How in the absence of imagery, and independent of it, can any combination of Wundt's entire tri-dimensional system of feelings account for the meaning of words and phrases or the mental categories formed in the perception of our figures? Pleasure and pain, tension and relaxation, excitement and repose, might conceivably combine to form complex emotions, moods, and a variety of non-representative mental states which accompany our processes of recognition, abstraction, analysis, etc. But that they should take over in their combination a function which is qualitatively distinct from any that is inherent in them as elements, is an unwarrantable assumption.

One might bring forward at this point the following objection: Your contention that there exist imageless mental contents is based in great measure on the experiments in which a common element was certainly perceived, although the subject did not at all know precisely what kind of a figure was present. But to conclude from such experiments that imageless ideas exist is not warranted, because the experiments may be explained without such an assumption. These experiments represent those cases in which the common element was never seen in the focal

[6] *Psychol. Studien*, 3, pp. 347-348 (note).

point of consciousness. But wherever it was perceived, however far in the background it might be, it gave rise to certain feelings of relaxation and restfulness, perhaps even of pleasure or displeasure. The peculiar combination of these feelings gave rise to the feeling of certainty that a figure was being repeated. This feeling had connected with it no visual imagery that the subject could recall. From such an analysis it is evident that from the lack of mental imagery you can not jump to the conclusion that there are imageless concepts.

Such an objection would not be based upon a complete analysis of the evidence. The existence of imageless concepts is not founded solely upon these rare cases—but also upon cases in which the subject was certain of parts of the figure that could easily have been drawn had any visual imagery been present. The subject described things such as points or curves or angles, which certainly could be pictured, but claimed to have no picture and could draw nothing that would represent his state of mind. Now, points and angles and curves are not mere feelings. And if they are present to the mind without imagery they are not images.

Furthermore, in the cases of recognition of figures without any knowledge whatsoever of their special nature, it is perfectly true that the basis of the subject's judgment to a large extent was some combination of feelings such as was mentioned in the supposed objection. But we must not forget that the basis of a judgment is not the judgment itself. And we must also remember that in all these cases there is in the subject's mind the abstraction, 'some kind of a figure' plus the knowledge that 'the figure was repeated.' The knowledge expressed by these two terms constitutes the judgment, 'some kind of a figure was repeated.' This judgment is not constituted by the feelings which evidenced the presence of a figure. It is based upon them, but it does not consist in them. It is therefore something over and above them. The elements into which this judgment can be analyzed are the abstraction 'some kind of a figure' and the knowledge that it 'was repeated.'

Since these elements are not feelings and are not mental images, there is nothing left in the current division of elementary mental processes into which they can be relegated except the acts of will. But certainly we can not place them there. We must therefore recognize the existence of another division of mental processes to which our thoughts and mental categories must be relegated. Consequently in the final product of abstraction there is an element distinct from imagery and feelings. This element, since it is the bearer of the meaning, is the kernel of the product and it may truly be termed the 'thought' or the 'concept.' Imagery and feeling may cluster about this concept; but as far as the imagery is concerned it is certainly lacking at times, as our experiments have shown. As to feeling, we can not say for certain on the basis of our experiments whether or not it is necessarily present.

The concept of the figures in our experiments, though distinct from imagery and feeling, was not itself an elementary process. It was manifestly compound in a number of instances. For one and the same figure was assimilated to several mental categories. It was a concept made up of several more elementary concepts. Between the concepts of which it was constituted there was a conscious bond. The sensation in being assimilated picked out its categories by the necessary process of its assimilation and these united to form a concept of the figure which the subject was afterwards enabled to analyze with more or less completeness.

If such is the case one may ask how was the first concept formed? Does man come into the world equipped with a whole system of mental categories by which he is enabled to perceive and understand the things about him? This question leads us on into a problem far beyond the limits of the present research. Our problem has been the analysis of the process of abstraction in the adult. The process of perception which is the initial stage of abstraction was found to be one of assimilating the sensation to previously formed mental categories. Whence originated these mental categories, is another problem. These

mental categories and their function in perception are facts. The origin of the mental categories, and the process of perception and abstraction in the child, are very different problems from our own. But ignorance of child psychology does not destroy the facts of adult mental life.

However, it may not be out of place to suggest a theory as to the origin of our mental categories. And this I would do as follows: As Külpe suggests,[7] the data of sense are perceived. There exists something of the nature of an 'inner sense'—a central consciousness which perceives the phenomena of the external senses. When consciousness first dawns the data of external sensations are perceived. Perhaps at first in the automatic life of the child the sensations that are perceived are more or less intermittent and vary in their nature. But every time a sensation arouses consciousness the child is aware of a change in its mental life. At first this change is not interpreted because there are as yet no mental categories. Every change is just an awareness. The child simply realizes that something has happened. And this realization develops into his first mental category. As time goes on, experiences multiply and the several different kinds of experiences make the child not only aware that 'something has happened' but that something of a more particular nature has happened. Something painful, something pleasant—something hot or something cold, etc. In this way he forms still further sets of mental categories into which his future experience is received. Out of these develop the categories of identity and diversity,—when, we do not know; nor is it necessary for us to settle this point here. But by a gradual determination of the most general of his mental categories—'something'—his experience grows and is assimilated. The first determinations are of very particular experiences. The most varied things are given one and the same name, simply because he has but a few general concepts and his sensations are assimilated by necessity to whatever categories may have been developed. The child's experience—his inner perception of a train

[7] *Bericht ü. d. I. Kongress f. exp. Psychol. in Giessen, 1904,* p. 67.

of similar mental events constitute a mental category which is his idea of those events. The first mental category, the child's awareness of something, enters though not consciously and explicitly into all his later concepts. Some of these later ones group together, and so on, until under the influence of language and education the events of the external world receive their interpretation.

SUMMARY.

We are now in a position to summarize the process of abstraction as revealed in our experiments.

1. The process of abstraction is initiated by the breaking up of the group presented for perception. In this breaking up of the group the common element becomes accentuated at the expense of the surrounding elements. These are not merely neglected, but are positively cast aside and swept more or less completely from the field of consciousness.

2. This breaking up of the group initiates the process of perceiving the common element. This is accomplished by assimilating to known mental categories the sensations perceived. Perception proceeds from that which is more general to that which is particular. The formation of a reproducible image represents a later and unessential stage of perception.

3. The retention in memory of the figure perceived depends in great measure on the method of memory. Memory by analysis and association has a very decided advantage over memory by imagery. The memory of the figure depends, furthermore, upon the focality of perception. The accuracy of memory decreases rapidly with the distance of the figure from the focal point in the act of vision by which it was perceived. The perception of new groups after a figure has been perceived has a tendency to obliterate it from memory.

4. The recognition of a figure once seen involves an element of certainty or uncertainty. Consequently there is implied in recognition assent or doubt, and therefore a judgment or a suspended judgment. In recognizing a figure any degree of certainty of recognition can accompany any degree of perfection in the perception of a figure, so that a subject may be certain of the repetition of a figure and still may have no knowledge as to what manner of figure it was—or the subject may know

all about a given figure and simply draw it as remembered, or as very doubtfully the common element.

5. The final product of abstraction, that which is perceived as common to many groups, is essentially a concept distinct from imagery and feeling. It is not an elementary concept, but represents the assimilation of that which is perceived by the senses to a more or less complex mental category, or perhaps to several such categories. These mental categories may be regarded as the results of past experience.

REFERENCES.*

ACH, NARZISS: *Ueber Willenstätigkeit und das Denken.* Göttingen, 1905.

ANGELL and HARWOOD: "Discrimination of Clangs for Different Intervals of Time." Part I, *American Journal of Psychology,* 1899-1900, Vol. XI, pp. 67-79. Part II, by Prof. Angell alone. *Op. cit.,* Vol. XII, pp. 58-79.

ASTER, E. VON: "Die psychologische Beobachtung und experimentelle Untersuchung von Denkvorgängen." *Zeitschrift für Psychologie,* 1908, Vol. XLIX, pp. 56-107.

BAGLEY, WILLIAM CHANDLER: "The Apperception of the Spoken Sentence." *American Journal of Psychology,* 1900-01, Vol. XII, pp. 80-134.

BERKELEY, GEORGE: *A Treatise concerning the Principles of Human Knowledge.* Vol. I, Fraser's, Oxford (1871) Edition of his works.

BIGHAM, JOHN: "Memory." *Psychological Review,* 1894, Vol. I, pp. 453-461.

BINET, ALFRED: *L'Etude expèrimentale de l'intelligence.* Paris, 1903.

BÜHLER, KARL: "Tatsachen und Probleme zu einer Psychologie der Denkvorgänge." *Archiv für die ges. Psychologie,* 1907, Vol. IX, pp. 297-365; 1908, Vol. XII, pp. 1-92.

——— ———: "Antwort auf die von W. Wundt erhobenen Einwände." *Archiv für die ges. Psychologie,* 1908, Vol. XII, pp. 93-123.

——— ———: "Zur Kritik der Denkexperimente." *Zeitschrift für Psychologie,* 1909, Vol. LI, pp. 108-118.

DAVIES, ARTHUR ERNEST: "An Analysis of Psychic Process." *Psychological Review,* 1905, Vol. XII, pp. 166-206.

DÜRR, E.: "Ueber die experimentelle Untersuchung der Denkvorgänge" *Zeitschrift für Psychologie,* 1908, Vol. XLIX, pp. 313-340.

GALTON, FRANCIS: "Composite portraits made by combining those of many different persons into a single resultant figure." *The Journal of the Anthropological Institute of Great Britain and Ireland,* 1879, Vol. VIII, pp. 132-144.

——— ———: "Generic Images." *Proceedings of the Royal Institute of Great Britain,* 1879, pp. 161-171.

——— ———: *Inquiries into Human Faculty,* New York, 1883.

GRÜNBAUM, A. A.: "Ueber die Abstraktion der Gleichheit." *Archiv für die ges. Psychologie,* 1908, Vol. XII, pp. 340-478.

HUXLEY, THOMAS H.: *David Hume,* New York, 1879.

* The list is not a bibliography of the subject but contains merely those works referred to in the present study.

KÜLPE, O.: "Versuche über Abstraktion." *Bericht über den I Kongress für experimentelle Psychologie in Giessen*, 1904, pp. 56-68.

MARBE, K.: *Experimentell-psychologische Untersuchungen über das Urteil.* Leipzig, 1901.

MAYER und ORTH: "Zur qualitativen Untersuchungen der Association." *Zeitschrift für Psychologie*, 1901, Vol. XXVI, pp. 1-13.

MESSER, AUGUST: "Experimentell-psychologische Untersuchungen über das Denken." *Archiv für die ges. Psychologie*, 1906, Vol. VIII, pp. 1-224.

—— ——: "Bemerkungen zu meiner 'Experimentell-psychologischen Untersuchungen über das Denken." *Archiv für die ges. Psychologie*, 1907, Vol. X, pp. 409-428.

MEUMANN, E.: "Ueber Associationsexperimente mit Beeinflussung der Reproduktionszeit." *Archiv für die ges. Psychologie*, 1907, Vol. IX, pp. 117-150.

MITTENZWEI, KUNO: "Ueber abstrahierende Apperzeption." *Psychol. Studien*, 1906-07, Vol. II, pp. 358-492.

MOORE, T. V.: "The Process of Recognition." *Atti del V. Congresso internazionale di Psicologia, tenuto in Roma dal 26 al 30 Aprile, 1905*, Roma, 1906, pp. 286-287.

ORTH, JOHANNES: "Gefühl und Bewusstseinslage." *Sammlung von Abhundlungen aus dem Gebiete der Pädagogischen Psychol. und Physiologie.* Edited by Ziegler and Ziehen, Vol. VI, No. 4, Berlin, 1903.

RIBOT, TH.: "Enquête sur les idées générales." *Revue philosophique*, 1891, Vol. XXXII, pp. 376, 388.

—— ——: *L'Evolution des idées générales.* Paris, 1897.

SCHULTZE, F. E. OTTO: "Einige Hauptgesichtspunkte der Beschreibung in der Elementarpsychologie. I. Erscheinungen und Gedanken." *Archiv für die ges. Psychologie*, 1906, Vol. VIII, pp. 241-338.

—— ——: "Beitrag zur Psychologie des Zeitbeweisstseins." *Archiv für die ges. Psychologie*, 1908, Vol. XIII, pp. 275-351.

SPEARMANN, C.: "The Proof and Measurement of Association between Two Things." *American Journal of Psychology*, 1904, Vol. XV, pp. 72-101.

TAYLOR, CLIFTON O.: "Ueber das Verstehen von Worten und Sätzen." *Zeitschrift für Psychologie*, 1906, Vol. XL, pp. 225-251.

WATT, HENRY J.: "Experimentelle Beiträge zu einer Theorie des Denkens." *Archiv für die ges. Psychologie*, 1905, Vol. IV, pp. 289-436.

WILTSE, S. E.: "Observations on General Terms." *Am. Journal of Psychology*, 1890-91, III, pp. 144-148.

WUNDT, WILHELM: *Grundzüge der physiologischen Psychologie.* 5th Edition, Leipzig, 1902-03.

—— ——: "Ueber Ausfrageexperimente und über die Methoden zur Psychologie des Denkens." *Psychologische Studien*, 1907, Vol. III, pp. 301-360.

—— ——: "Kritische Nachlese zur Ausfragemethode." *Archiv für die ges. Psychologie*, 1908, Vol. XI, pp. 444-459.

APPENDIX I.

THE INFLUENCE OF ASSOCIATION ON PERCEPTION.

In the course of the experiments a number of cases occurred in which the subject's drawing of the common element differed from the actual figure in such a way that the error was evidently due to the association that was reported. Some of these cases are given below. The drawings given under the heading "subject's drawing" reproduce the essential characters of those made by the observer. They are not however exact reproductions of his drawings.

COMMON ELEMENT.	SUBJECT'S DRAWING.	ASSOCIATION.
		"Wurst."
		Omega.
	The subject drew the figure correctly at first. He then changed his mind and drew a second figure with a double curve,  saying that this was more correct. He said the figure looked like a (*Läufer*). His second figure does in fact resemble the bishop in some forms of chessmen.	
		Mushroom.
		Two half-moons.
		Open scissors.
		Apple.

These errors lend additional evidence to the theory of perception advanced in the body of this work. The actual imagery arising from the figure itself is not the first thing noticed. It fits into and is interpreted in the light of the subject's past experience. The association comes into the subject's mind first. He sees *that*, and interprets the data of vision in its light before the true image is perceived. Had the series in which these errors occurred been sufficiently long there can be no doubt but that the error of assimilation would have been corrected. The true image which was constantly being impressed upon the retina would have eventually been noticed as it was in itself. But because perception does not consist in merely seeing with one's eyes but in interpreting the data of the senses, such errors as the above are not only possible but natural.

APPENDIX II.

GENERIC IMAGES.

What looked like a fusion of mental images occurred twice in the course of the experiments. Such fusions are interesting in view of the generic image theory of ideas. The first case was less evidently one of fusion. The disk was inaccurately made. The accompanying figure occurred as the common element, now in one, now in another of the two positions as given.

The result was that the two circles were drawn correctly. Just what was in the inner one remained doubtful.

The second case seemed evidently a fusion of mental images. It occurred on a disk with no common element. The accom-

panying two figures occurred several expositions apart. The ~~subject drew the common element. The outline he said was~~ subject drew  as the common element. The outline he said was certain, the dotted inner line was doubtful. Since the two figures appeared several expositions apart there can be no question at all of a retinal fusion. The phenomenon must be due to some central cause.

Just such cases as these resemble very closely those postulated by the Huxley-Galton theory of general ideas. The common features are deeply impressed and therefore retained; the variable, but faintly, and are neglected. There is however a very important difference between the universal idea and such "generic images," as were found in the entire course of the experiments.

In the formation of a general idea there is a kernel picked out as constantly recurring and therefore essential, while that

which is variable is neglected and forgotten, or recognized as unessential.

If in our experiments there was any fusion of images at all, that which was common was indeed clearly impressed,—but that which was variable was neither neglected nor forgotten but remained obscure and doubtful.

At all events the extreme rarity of the phenomenon postulated by the Huxley-Galton theory shows that it cannot be the usual way in which we form our concepts,—not even those of sensible objects. The analysis of abstraction made possible by the experiments points to a process that has little to do with composite photography.

Transmitted December, 1909.

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 3, pp. 199-268, pls. 1-4 February 23, 1914

THE JUDGMENT OF VERY WEAK SENSORY STIMULI

WITH SPECIAL REFERENCE TO THE ABSOLUTE THRESHOLD OF SENSATION FOR COMMON SALT

BY

WARNER BROWN

CONTENTS

THE JUDGMENT OF VERY WEAK SENSORY STIMULI

I

Introduction

The term *absolute threshold of sensation* occurs very frequently in the writings of psychologists, and it is generally treated as if it were a technical term of precision. But it has appeared to the present writer that the concrete experiences upon which the notion of the absolute threshold of sensation is founded have received very little consideration, and remain, for the most part, unanalyzed. The experiences in question are those in which a stimulus is presented which is so weak in intensity, or so little different in quality from the general background upon which it is presented, that there is some question whether or not it can be perceived.

The attempt to produce such an experience under experimental conditions is beset with serious difficulties. No one is ever free from all sensation, nor is it possible to impress a weak stimulus upon a person in the absence of other sensations which resemble the one produced by the stimulus. Hypothetical exceptions may be found in infancy or in the return of consciousness after a period of unconsciousness, but in normal life none of the sense-organs is ever so perfectly adapted that it fails to yield some sensation. The minimum visible light comes as a change in a field of vision always abounding in sensation. No one ever heard a faint sound beginning out of utter stillness; to listen for such a sound is to become aware of countless sounds produced both within and without the body. The cutaneous senses, as well as smell and taste, present the same difficulties only a little less obtrusively; these senses do reach states of almost perfect adaptation so long as we do not attend too closely in their direction; but once we begin to experiment upon them, we are overwhelmed

with sensations which refuse to be eliminated. The least perceptible pressure or pain, or temperature, or smell or taste is only a modification of a complex of sensations already present.

Such being the case, it might be presumed that an examination of the conditions determining the threshold of difference would cover the case for the absolute threshold as well. There is, however, a simple and fundamental distinction between the two cases. The threshold of difference is found between two stimuli of almost the same quality, but differing slightly in intensity. The absolute threshold marks a difference of quality within a field which does not change appreciably in intensity. The difference-threshold is observed only by comparison with a standard sensation of some kind; the absolute threshold must be observed without the help of such a standard. Moreover, every one who has ever investigated the matter knows that the attitude of the observer is radically different in determining the two thresholds. When examining an apparently tasteless substance to discover whether or not it contains salt (search for the absolute threshold), one ignores the relative intensities of such sensations as are clearly present, seeking to detect the quality of those which are dimly suspected. When concerned with the threshold of difference, the observer withdraws his attention from everything else in order to apprehend as clearly as he can the relatively conspicuous sensations with which he is directly concerned, and about whose existence there is not the slightest doubt.

A fair determination of an absolute threshold depends upon the fulfilment of two chief conditions. First, before stimulation the sense-organ must be in a state of adaptation to its surroundings, and for purposes of practical experimentation it must be possible to produce this state of adaptation quickly. Second, it must be possible to introduce a relatively simple, familiar, and distinct stimulus which can be easily graded quantitatively. Under these conditions the expected sensation can be observed with a minimum of comparison with other sensations, and with a maximum of difference in quality between itself and such other sensations as are present.

Without entering into details, it may be said that the sense of taste satisfies these conditions as well as any of the other senses. It is well known that the organs of taste are almost completely adapted when not stimulated by foreign substances taken into the mouth. Only in morbid states of the organism are there tastes, sweet, bitter, or sour (not, however, salt), due either to some disturbance of the sense-organs or to the presence of unusual products in the saliva. None of the other senses is so free from subjective sensations and so perfectly passive. The organs of taste are sheltered from outside disturbances by the strong surrounding walls of the mouth. They are stimulated from within by a fluid of a very uniform consistency and savor.[1] The amount of salt constantly present in the saliva is considerable compared with the small additional amounts which can be detected by taste. None of the other senses has the benefit of constant stimulation by a uniform irritant. In a word, adaptation is secured by freedom from occasional stimulation and by a weak, uninterrupted stimulation. In practice it is not possible, however, to avoid comparison with a standard; and this is as true of taste as of any of the other senses. A single sample stimulus can be judged after a period of comparative passivity of the organ; but if further judgments are to be made within a reasonable period of time, they are subject to disturbance from the traces of the preceding stimulus or from the mechanical means employed to remove such traces. Traces of any substance taken into the mouth remain until removed by the slow flow from the salivary glands or by the help of a rinse. It is customary to use pure water as a mouth-wash to remove traces of previous stimuli, and water is thought of as being itself tasteless. But all water when closely examined yields a taste.[2] It appears that the only way to insure perfect quiescence in the taste-organs is to allow them to remain

[1] See W. Nagel, *Handbuch der Physiologie des Menschen* (Braunschweig, F. Vieweg u. Sohn, 1907), vol. 2, p. 521; C. Oppenheimer, *Handbuch der Biochemie des Menschen und der Tiere* (Jena, G. Fischer, 1910), vol. 3, pt. 1, p. 27.

[2] On the taste of water, see Appendix I.

for a considerable time unexcited by any substance other than the normal saliva.[3]

The second requisite of a good threshold-stimulus—that its quality be familiar and at the same time clear and distinct and easily regulated in intensity—is satisfactorily fulfilled by any of the four chief taste qualities; but salt possesses certain advantages over the other three. Salt sensations are not produced accidentally by stimulation with water.[4] Weak salt solutions have no disagreeable effects, and even after repeated tasting produce none of the strong feelings which attend bitter, sweet and sour. Salt solutions are easily prepared and keep well.[5] A large number of previous determinations of the threshold are available in the case of salt, while for the other taste qualities such old determinations are fewer and rest upon stimulation by such a variety of substances that very few direct comparisons can be made.[6] The previous determinations seem to be more consistent with each other for salt than for the other taste qualities, and this fact would seem to indicate that fewer difficulties or errors are encountered in administering this stimulus. The reaction time for salt is shorter than for the other qualities, and the discrepancy between the simple reaction time and the time for discriminating salt is less, and this would seem to indicate that

---

[3] Moreover perfect strictness of method would require that time be allowed in which the saliva itself may return to a normal state after the disturbances of its constitution caused by the introduction of foreign substances into the mouth. See Pavlov, I. P., *The Work of the Digestive Glands*, tr. by W. H. Thompson (2nd Eng. ed., London, Charles Griffin and Co., 1910), p. 71.

[4] Kiesow, F., *Philosophische Studien*, vol. 10 (1894), p. 528; Titchener, E. B., *Textbook of Exp. Psychol.*, vol. 1, pt. 2, p. 100; and see Appendix I, On the Taste of Distilled Water.

[5] There are various technical difficulties in the way of standardizing sugar for such work, and the curious differences between sugar and saccharine discovered by Lemberger, *Arch. f. d. ges. Physiol.*, vol. 123 (1908), p. 293, are sufficient warrant for rejecting sweet tastes for the present work, in which simplicity in the stimulus is the first requisite.
Acid stimuli require such extraordinary dilutions for the threshold that they are inconvenient. Richards, *Amer. Chem. Jour.*, vol. 20 (1898), p. 121, finds that hydrochloric acid tastes sour as long as there is any free HCl present, and that the sense of taste gives finer discriminations than some so-called quantitative methods.

[6] On the determinations of the threshold of intensity for salt, see Appendix II.

the salt quality, when perceived, is more clear or unambiguous than the others.[7]

As a further point in favor of using salt as a stimulus it may be noted that the tongue is about equally sensitive all around to salt, while for the other qualities some portions are more sensitive than others.[8] On this account it makes less difference with salt if the stimulus does not always strike first on the same part of the tongue.

---

[7] Kiesow, *Ztsch. f. Psychol.*, vol. 33, (1903), p. 456, gives the following reaction times:

Salt,	0.308 sec.
Sugar,	0.446 sec.
HCl,	0.536 sec.
Quinine,	1.082 sec.

See also von Vintschgau and Hönigschmied, Pflüger's *Arch. f. d. ges. Physiol.*, vol. 10 (1875), p. 42, and the article by von Vintschgau in Hermann's *Handb. d. Physiol.*, vol. 3, pt. 2 (1880), p. 205.

Notwithstanding the fact that the salt solutions used were less intense than the other tastes, von Vintschgau, *Arch. f. d. ges. Physiol.*, vol. 14 (1877), p. 550, found that the time required to discriminate salt from water was less than the time required to discriminate the other tastes from water, as follows:

Salt,	0.276 sec.
Acid,	0.332 sec.
Sugar,	0.384 sec.
Bitter,	0.413 sec.

Salt shows the least difference between the maximum and minimum reaction times (*ibid.*, p. 551). The time for discriminating between two tastes is shortest when one of these tastes is salt (*ibid.*, p. 554).

[8] Wundt, *Physiol. Psychol.*, vol. 2 (1910), p. 62; Haenig, *Philos. Studien*, vol. 17 (1901), pp. 606–608.

II

STATEMENT OF THE WORK DONE

Two sets of solutions of common salt[9] in water[10] were prepared in quantity once for all. One set, called the "strong" solutions, contained the following sixteen members, the figures denoting the per cent of salt to water by weight, i.e., the number of grams NaCl per 100 cubic centimeters H_2O.

Name of solution	Strength, per cent
0	0.0%
1	0.1%
2	0.2%
3	0.3%
4	0.4%
and so on, by steps of 0.1%, to	
15	1.5%

The "weak" series contained the following fifteen members:

Name of solution	Strength, per cent
0	0.0%
1/4	0.025%
2/4	0.05%
3/4	0.075%
1	0.1%
5/4	0.125%
and so on, by steps of 0.025%, to	
14/4	0.35%

For each observer a set of small bottles was provided, one for each solution in the series, and for each bottle a common medicine-dropper, or pipette, the tube of which slipped snugly into the bottle,

[9] The salt used was that prepared by the J. T. Baker Chemical Company of Philipsburg, New Jersey, under the label "Sodium Chloride, C.P., M.W. 58.5." It is said to contain, besides NaCl, 0.0002 per cent iron, and 0.005 per cent sulfate.

[10] The water used was taken from the still of the Department of Chemistry in this University. It is distilled once from copper and contains some traces of copper salts, but is sufficiently pure for use in ordinary quantitative analysis.

and of which the rubber bulb closed the neck of the bottle and prevented evaporation. Before a sitting, these small bottles were filled from the stock and brought into the operating room in time to allow their contents to come to the temperature of the room.[11]

After work had once begun the observers reported regularly once or twice a week, or daily, as the case might be, at the same time of day. They did not work if they felt out of sorts or had colds. All judgments were recorded. The observer was seated so that he could not see the bottles.[12] The stimuli were administered in chance order, but in several series a record was kept of this order so that it was possible to tell what stimulus and what judgment preceded any particular stimulus, and in these latter cases care was taken to see that each solution should come first in a series as often as did any other solution. Otherwise a chance order was followed.

When the observer had seated himself and rinsed his mouth (using as much or as little water for this purpose as he chose),[13] the experimenter took one of the small bottles, pumped with the pipette to stir up the solution, filled the pipette till it contained one cubic centimeter, and handed it to the observer, who opened his mouth and emptied the pipette on to the center of his tongue,

---

11 Authorities differ as to the effect of the temperature of the solution upon the sensitivity of the observer. I am not prepared to say that the threshold would be affected by using blood heat instead of the temperature of the room, but I am ready to admit, in the light of occasional introspections from my observers, that it would be worth while, for the sake of precision, to take the pains to keep all of the fluids used at a fixed temperature of about 30° C and also to regulate the room temperature carefully. On the other hand, I wish to state that my own *a priori* objections to using a fixed temperature were, that it would appear warmer on a cold day and cooler on a warm day, and that if it were as high as blood temperature it would make the stronger salt solutions somewhat nauseating. I kept all the fluids at the temperature of the room, which was never hot or cold, and so they bore a constant relation to the physiological zero point of the observer's skin. See Kiesow, *Philos. Studien*, vol. 12 (1896), p. 464.

12 The bottles were not labeled in order, but according to an arbitrary system of which even the experimenter did not, then and there, have the key.

13 The amount of rinse water is a minor factor, and yet it may assume some importance when an effort is being made to keep the conditions perfectly uniform from one stimulation to the next. I rather thoughtlessly followed precedent in allowing each observer to use his own discretion in this matter.

then closed his mouth, moved the solution about with his tongue and spit it out.[14] The observer then expressed his judgment by "yes" or "no," meaning "salt" or "not salt."[15] Some observers expressed judgment by a shake of the head and omitted spitting out the solution, taking the rinse water while the solution was still in the mouth. All but the most conscientious observers occasionally swallowed the rinse water, almost involuntarily (and with it the one dose of salt) as the result of thirst, induced partly by

[14] See Appendix II.

[15] No doubt objection will still be made to the exclusion of doubtful answers. In reporting my experiments on "The Judgment of Difference" (page 28 of this volume of the present series), I tried to justify the exclusion, in general, of doubtful answers when these answers represent, not a distinct type of judgment, but rather the absence of any explicit judgment at all. It would be useless to repeat those arguments here; but there is one peculiarity of the present problem that makes the case more than usually *obvious. Our whole interest in the present situation lies* in determining whether the observer does or does not have a particular sensation. A doubtful answer from him has no significance at all. Nominally it signifies a sort of intermediate state between sensation and absence of sensation. The answer "I don't know" to the question "Do you taste salt?" means either that no salt was tasted or that the observer was incapable at the time of making a rational judgment. Under no circumstances can it mean that he had a half taste of salt or that what he tasted was half salt and half water. Psychophysics must run wild in its attempt to measure sensations before it can undertake to count up fractions of sensations.

A good example of the absurdity arising from the admission of doubtful answers can be seen in the work of Lemberger, *Arch. f. d. ges. Physiol.*, vol. 123 (1908), p. 304, in comparing sugar with saccharine. In only eight out of a total of 40 cases in one series was the sensation reported, and yet this 20 per cent of positive judgments mounts to 60 per cent by the inclusion of the doubtful cases, of which there were 32. In other words, 16 positive cases are derived from the doubtful column, when there were only 8 truly positive cases. Two thirds of the positive judgments reported for this solution arose from situations in which the observer was really unable to make any judgment at all. This is merely a horrible example, taken from what is in many ways an admirable piece of work, of the dangers of allowing states of uncertainty to figure in a statistical array of explicit judgments. It seems to me that there is no way of getting judgments upon a concrete situation except to ask for them. Surely it is not requiring too much to ask a person to report whether he tastes or does not taste salt. If he does not taste salt, all he has to do is to say so. As a matter of fact, none of my observers, many of whom were entirely new to work of this sort, had the slightest difficulty in meeting these requirements. They were told beforehand that there were some of the solutions that tasted salt and others that did not, and that they were to decide which were which, and that they must decide in every case. They decided every time, and there was seldom any hesitation. The tables show that some of them, at any rate, were exceedingly consistent in these decisions.

Some light may be obtained on the true nature of the "doubtful"

the sight and taste of the water itself and partly by the continual loss of saliva and consequent dryness of the throat.

While the mouth was being rinsed the experimenter was preparing the next pipette and entering the judgment. As soon as the rinse water had been spit out, the next pipette was handed to the observer. This routine was continued until the series of fifteen or sixteen solutions had been gone through once; then a two-minute rest was taken, and then the routine was resumed.[16]

answer by an inspection of one of the tables published by Camerer, *Ztsch. f. Biol.*, vol. 21 (1885), p. 601.

The distribution of answers found by Camerer when there were 3 bitter, 3 salt, and 2 water cases in each series. The judgments are given in per cent out of 160 for each solution for each of two observers. The upper number refers to Observer I, the lower to Observer II. The method of work was "unwissentliches, gemischt" as in our own experiments.

	Salt Solutions			Quinine Solutions				
Judgment	No. 2	No. 3	No. 8	No. 1	No. 1.5	No. 3	Water	Water
"Salt"	27.5	66.9	98.7	....	....	....	....	0.6
	52.5	71.9	98.7	....	....	....	....	....
"Bitter"	1.9	....	....	41.2	55.6	81.9	10.0	1.9
	0.6	....		64.4	83.1	90.6	1.2	0.6
"Water"	23.1	9.4	....	28.7	20.6	6.2	81.1	85.0
	15.6	3.7	.	6.9	1.9	0.6	92.5	97.5
	41.9	21.2	1.2	1.2	1.2	....	0.6	3.7
	22.5	19.4	....	0.6	....	....	....	....
"Uncertain, perhaps bitter"	2.5	1.2	....	26.2	21.9	10.0	7.5	5.6
	0.6	0.6	....	22.5	10.0	7.5	5.0	1.2
"Not water, and and not surely salt or bitter"	3.1	0.6	....	2.5	0.6	1.9	0.6	3.1
	8.1	4.4	1.2	5.6	4.4	1.2	0.6	....
No judgment	....	0.6	....	....	....	....	....	....
					0.6	....	0.6	0.6

Here three salt, three bitter, and two water stimuli occurred in the series. In this case the judgments "uncertain, perhaps salt" or "uncertain, perhaps bitter" should really be listed as correct judgments. They are as apt to be right as the "surely salt" or "surely bitter." The judgment "not water and not surely salt or bitter" is correct in so far as it stands for a sensation which is positive but not clear as to quality. "No judgment" gives practically no returns at all; there was always a judgment. *Under these circumstances* it becomes evident that the doubtful answers, instead of being half right and half wrong, are nearly all right.

Under ordinary circumstances, when the alternative of "salt" or "not salt" is offered, the judgment "uncertain" means "uncertain, perhaps salt" as in this table of Camerer's. It is absurd to decide *a priori* that half or any part of these judgments are wrong. The rational thing to do is to exclude such judgments from the start.

[16] The interval between successive stimulations was much less than is customary in such experiments. Careful preliminary experiments indi-

The experimenters fall into two groups. The observers in the first group were students in the University of California Summer Session of 1910 who were doing their first work in experimental psychology. There were eight of them, of whom five were mature and experienced persons; the others were college students. They received no preliminary training and were allowed only a few trials before they began the actual work of the experiment. Their observations were made under my eye and with my help by fellow-students. They each passed forty judgments on each of the sixteen "strong" solutions (see p. 206). The data from these observations are given in the curves of Plates 1 and 2, and in Table XVIII. In the same group with them are the data from observer Libbin, a graduate student without previous laboratory training. In the case of this observer, however, an extended period of tentative experiment with other intensities of salt and other modes of judgment preceded the forty judgments for which the curve is drawn.

The experiments of the second group were all performed by the writer himself during the year 1910-11. In these cases the order of succession of the judgments was recorded. Observer Noteware, who had already made forty rounds of judgments in the summer on the "strong" series, now made forty more the

---

cated that judgments could be made in rapid succession without any more difficulty than when they were made at longer intervals. The rate used was that at which the work of tasting and rinsing on the part of the observer and recording on the part of the experimenter could be carried on without interruption, for a series of fifteen or sixteen judgments. At the end of such a series a rest of two minutes was taken. Some eight series of fifteen judgments were usually made in a fifty minute sitting, or about three judgments per minute. This rate seems fast compared with the leisurely methods of some of the early investigators (Camerer's observers studied vocabularies during intervals of two and a half minutes) but I feel confident that it is wise to work on such experiments continuously with sustained attention instead of allowing the observer's mind to wander off into other paths from which it must again soon be recalled to work. Continuously sustained attention demands far less energy than continuously renewed attention to such monotonous tasks; the number of judgments made at a sitting is greater, and there is no evidence that they are in any respect inferior.

While we ought, tentatively, to adopt the most efficient programme for such work, refusing to use more time than is demonstrably necessary, direct experiments are required to discover the limits within which it is desirable to "speed up" in psychological laboratories.

following spring on the "weak" series (see Plate 3, fig. 3.) Observer Kohlberg, a senior student working for the second year in the laboratory, made eighty rounds of judgments on the "strong" series without preliminary practice (see Plate 1). Observer von der Nienburg, of like training, after preliminary practice made 120 rounds of judgments (1800 in all) on the "weak" series, of which only the last hundred rounds are regarded in some of the tables (see Plate 3, fig. 1.) She later made forty-five rounds on the "strong" series, of which only the last forty have been counted (see Plate 2.) Observer Levy, a graduate student with several years' experience in the laboratory, made eighty rounds on the "weak" series after a good deal of practice on somewhat stronger and weaker stimuli, devoted to determining the best intensities with which to work (see Plate 3, fig. 2.)

III

DIFFICULTIES TO BE CONSIDERED IN THE EXPERIMENT

A. PRACTICE, ADAPTATION, AND FATIGUE

Tables I–V show for four observers the number of series of fifteen or sixteen judgments made at each sitting and, at the bottom, the average number of positive judgments given in the course of each of these successive series. These averages are all shown together in Table VI. If the very rapid rate of presentation exercised a bad influence on the observer, we might expect it to appear in these tables as a fatigue effect. That is, the number of positive judgments might steadily increase (or decrease)

TABLES SHOWING THE NUMBER OF POSITIVE JUDGMENTS IN EACH OF 16 (OR 15) STIMULI ON EACH DAY OF WORK

TABLE I

OBSERVER MISS KOHLBERG

16 Strong Stimuli

	Round	1	2	3	4	5	6	7	8	Average
Sept.	21	10	12	10	14	....	....	....	....	11.5
	26	9	10	10	9	9	....	....	....	9.4
Oct.	3	10	10	11	11	11	10	10	6	9.9
	10	10	5	8	7	7	10	....	....	7.8
	17	11	7	8	7	7	9	....	....	8.2
	31	10	7	8	8	7	10	8	....	8.3
Nov.	7	9	9	7	9	10	9	9	....	8.9
	14	8	5	9	9	8	9	8	....	8.0
	21	8	9	9	9	6	7	7	....	7.9
	28	8	8	8	8	7	6	7	6	7.3
	30	11	9	9	8	9	10	8	11	9.4
Dec.	5	10	8	8	6	6	6	9	....	7.8
Av. per round		9.5	8.3	8.8	8.8	8.0	8.6	8.3	7.7	8.6

TABLE II

OBSERVER MISS VON DER NIENBURG

16 Strong Solutions

Round	1	2	3	4	5	6	7	8	9	10	Average
Nov. 24	10*	10*	10*	11*	11*	11	12	10	....	....	10.6
28	12	14	15	15	15	14	15	15	15	....	14.4
30	14	15	14	13	13	13	15	12	14	....	13.7
Dec. 5	15	15	14	16	15	14	14	15	14	....	14.9
7	15	15	15	16	13	13	14	15	14	16	14.6
Average per round	13.2	13.8	13.6	14.2	13.4	13.0	14.0	13.4	14.2	16.0	13.7

* Not included in other tables.

TABLE III

OBSERVER MISS VON DER NIENBURG

15 Weak Solutions

Round	1	2	3	4	5	6	7	8	9	Average
Sept. 26*	14	13	12	11	14	....	....	....	....	12.8
28*	10	11	8	8	5	6	6	8	....	7.8
Oct. 3*	10	7	5	9	8	9	7	....	....	7.9
5	5	3	5	2	4	3	3	5	3	3.66
10	4	5	4	6	6	6	4	4	....	4.88
12	3	5	5	4	4	4	3	....	....	4.00
17	4	3	4	2	2	4	4	....	....	3.27
24	3	3	2	3	3	2	2	....	....	2.59
26	1	1	2	1	0	3	2	2	....	1.50
31	3	2	2	2	2	2	3	....	....	2.28
Nov. 2	3	2	3	2	2	1	1	....	....	2.00
7	2	2	3	3	2	1	2	3	....	2.25
9	4	6	2	3	1	3	4	4	....	3.38
14	5	3	4	3	4	5	2	1	....	3.38
16	6	4	4	4	4	3	5	4	....	4.00
21	6	4	1	5	4	2	3	4	....	3.63
Average per round	3.77	3.31	3.15	3.08	2.92	3.00	2.92	3.38	3.00	3.18

* The observations made on these days are not included in other tables, nor in the averages at the foot of this table.

TABLE IV

OBSERVER MR. LEVY

15 Weak Solutions

	Round	1	2	3	4	5	6	7	Average
Sept.	30	4	8	7	5	7	7	.	6.3
Oct.	8	5	6	5	4	5	7	5	5.3
	12	8	6	9	5	8	7	9	7.4
	24	6	10	7	6	5	6	.	6.7
	29	8	9	5	10	7	10	..	8.2
Nov.	3	9	4	6	6	8	...	..	6.6
	18	8	7	6	..	..	..	..	7.0
	25	7	8	6	6	7	7	..	6.8
Dec.	2	8	6	6	6	7	5	..	6.3
	9	6	8	8	6	6	..	..	6.8
Feb.	3	8	6	7	8	8	..	..	7.4
	10	7	7	6	8	5	7	..	6.7
	17	6	5	10	7	6	9	7	7.1
	24	5	5	7	7	6	..	.	6.0
Average per round		6.8	6.8	6.8	6.5	6.5	7.2	7.0	6.75

TABLE V

OBSERVER MISS NOTEWARE

15 Weak Solutions

	Round	1	2	3	4	5	6	7	8	Average
Feb.	24	6	7	4	7	7	9	5	6	6.4
Mar.	3	7	7	6	9	7	8	13	6	8.0
	10	6	8	10	5	7	7	6	7	7.0
	17	9	10	7	9	10	10	10	11	9.5
	24	9	11	8	9	8	7	8	7	8.4
Av. per round		7.4	8.6	7.0	7.8	7.8	8.2	8.4	7.6	7.85

with the advance of the hour. One observer only, Miss von der Nienburg, and in only one of her two series (Table III), gave less and less positive judgments from round to round. Observer Kohlberg (Table I) gave more positive judgments on the first round, but showed no decrease after the second round. The other two observers showed no regular change at all during the sitting.

Reference will be made later (p. 218) to certain purely "central" or subjective factors which influence the attitude of the observer and determine roughly the proportion of positive and negative judgments in a series. For observer von der Nienburg these factors tend to reduce the number of positive judgments in the "weak" series and to increase them in the "strong" series. These factors account for a progressive change in the proportion of positive judgments during the course of the sitting; and such an account holds for both of the series with this observer, whereas the effect can be ascribed to fatigue in only one of the series.

TABLE VI

SHOWING THE AVERAGE NUMBER OF POSITIVE JUDGMENTS OUT OF 15 OR 16 GIVEN ON EACH SUCCESSIVE ROUND DURING AN HOUR'S SITTING

	Round	1	2	3	4	5	6	7	8	9
Miss K.	out of 16 strong solutions	9.5	8.3	8.8	8.8	8.0	8.6	8.3[1]	7.7[2]	......
Miss v. d. N.	out of 16 strong solutions	13.2	13.8	13.6	14.2	13.4	13.0	14.0	13.4	14.2[3]
Miss v. d. N.	out of 15 weak solutions	3.77	3.31	3.15	3.08	2.92	3.00	2.92	3.38[4]	....
Mr. L.	out of 15 weak solutions	6.8	6.8	6.8	6.5	6.5	7.2[5]	7.0[6]	....	......
Miss N[7]	out of 15 weak solutions	7.4	8.6	7.0	7.8	7.8	8.2	8.4	7.6	......

[1] Results obtained on only 8 of 12 sittings.
[2] Results obtained on only 3 of 12 sittings.
[3] Results obtained on only 4 of 5 sittings.
[4] Results obtained on only 8 of 13 sittings.
[5] Results obtained on only 9 of 14 sittings.
[6] Results obtained on only 3 of 14 sittings.
[7] Five sittings.

On the whole, there seems to be no reason for supposing that the rapid succession of stimuli tended to dull the sensory acuity of any of the observers.

The further question remains whether an after-effect persisted from one stimulus to the next, so that the successive stimulations interfered with one another.

B. CONTRAST

It might be supposed that if the stimuli were applied in too rapid succession there would be disturbing positive after-effects, i.e., that the effects of strong positive stimuli would persist after

Table VII. CONTRAST

Influence of the Strength of the Preceding Stimulus upon the Judgment

The last column of percentages shows the proportion of positive judgments from all cases, without regard to the preceding stimuli or judgments. The next to the last column shows the proportion of positive judgments when contrast conditions are present and when they are not.

Miss K., 80 rounds of 16 strong solutions	496	positive	judgments	follow	586 weaker*	stimuli,	or 84.6%	against 54.4%
	157	”	”	”	614 stronger	”	or 25.6%	normally
Miss v. d. N., 40 rounds of 16 strong solutions	289	”	”	”	293 weaker	”	or 98.6%	against 87.5%
	236	”	”	”	307 stronger	”	or 76.9%	normally
Miss v. d. N., 40 rounds the first 6 of the above 16	24	”	”	”	28 weaker	”	or 85.8%	against 67.0%
	126	”	”	”	196 stronger	”	or 64.2%	normally
Miss v. d. N., 100 rounds of 15 weak solutions	240	”	”	”	698 weaker	”	or 34.5%	against 22.4%
	73	”	”	”	702 stronger	”	or 10.4%	normally
Miss L., 80 rounds of 15 weak solutions	274	”	”	”	553 weaker	”	or 49.5%	against 45.8%
	239	”	”	”	567 stronger	”	or 42.2%	normally
Miss N., 40 rounds of 15 weak solutions	166	”	”	”	285 weaker	”	or 58.2%	against 52.3%
	127	”	”	”	275 stronger	”	or 46.2%	normally

* "Weaker" means a stimulus weaker than the one being judged.

their removal and be carried over to the next stimulus. Table VII shows that in every instance in which a record was kept of the order of the successive judgments a negative after-effect, or what is ordinarily called contrast, appeared. If the solution which is being tasted is stronger than the one preceding, there is a tendency to call it salt; but if it is weaker than the preceding there is an equally strong tendency to call it water.[17] It should be observed that three of the four observers were judging of very weak stimuli, and that for two of them (the last two in the table) none of the stimuli were clearly salt, although both of the observers often *said* that salt was clearly present.

Since observer von der Nienburg gave a very large proportion of positive answers in the "strong" series, a separate presentation is made of the results after excluding the ten strongest of these solutions. Only one negative judgment occurred on any of these ten solutions. For the remaining six solutions the proportion of positive answers is normally 67 per cent, but when the preceding stimulus has been weaker the proportion of positive answers rises to 85.8 per cent.

---

17 See p. 219 for a further statement of the method of securing these facts.

IV

THE DEPENDENCE OF SENSORY JUDGMENTS ON CENTRAL CONDITIONS

The chief aim in undertaking the present group of experiments was to show that every judgment we make, no matter how simple the subject-matter, is conditioned to a very large extent by inward or "subjective" factors. Such factors are more properly designated "central" in contradistinction to the peripheral factors which, in one way or another, compel the individual to make a reaction. The central factors may be thought of as the condition of the organism itself modifying its reaction. Obviously different organisms react differently to the same stimulus, and one reason is to be found in the different capacities which different individuals have. So, too, the same organism does not always react in the same way to stimuli which are apparently identical, and in this case the difference in reaction may be thought of as due to the different condition of the organism. Such a difference in condition constitutes a "central" factor in the reaction.

In the case of the judgment of difference[18] it was found that small differences in the task assigned, or small differences in the mode of reaction demanded, had the effect of altering considerably the observer's judgment in situations which were otherwise precisely equivalent. Moreover, a change in the arrangement of the subject-matter so that a particular case appeared in different company so altered the general attitude of the observer that his judgment would be affected. When, for instance, all the differences between the weights which he was judging were small, he could judge very small ones competently; but let the same task be given when coarser differences were being

[18] See "The Judgment of Difference," p. 52 of this volume.

judged, then the central conditions were no longer favorable for such accurate judgment.

The judgment of difference is a comparatively complicated undertaking. The judgment of sensory stimuli is generally supposed to be much simpler. Is it still true in the simpler case that central conditions play an active part in the formation of judgments? There is no question here of the distinction between sensation and perception or of the association processes necessarily involved in the recognition of a stimulus. The point is whether there are changes in the attitude of the observer which affect his capacity to perform his part in sensation, no matter what that part is.

A. The Influence of Successive Judgments on One Another as a Disturbing Central Factor

In the second group of experiments, embracing one hundred rounds of weak stimuli and forty rounds of strong stimuli with observer von der Nienburg, eighty rounds of strong stimuli with observer Kohlberg, forty rounds of weak stimuli with observer Noteware, and eighty rounds of weak stimuli with observer Levy, a record was kept of the order of presentation of the stimuli. A sample record sheet for one of these 340 series is shown on page 220. From these original record sheets, tally sheets were made up, of which a sample appears on page 221, showing the complete distribution of all the judgments passed on each stimulus. Taking all the positive judgments for that solution, the tally sheet shows how many were preceded by solutions stronger than the one in question and how many were preceded by a solution weaker than the one in question. Then, further, the tally sheet shows the number of preceding cases which were judged salt and the number which were judged not salt. And so for the various

combinations, so that the footing up of the tally sheets for an individual observer gives all the judgments in the following categories:

A. Positive judgments, "salt."
B. Negative judgments, "not salt."
A1. Positive judgments preceded by a *positive judgment* on a *solution stronger* than the one in question.
A2. Positive judgments preceded by a *negative judgment* on a *solution stronger* than the one in question.
A3. Positive judgments preceded by a *positive judgment* on a *solution weaker* than the one in question.
A4. Positive judgments preceded by a *negative judgment* on a *solution weaker* than the one in question.
A5. Positive judgments preceded by *nothing*, that is, the stimulus was given after a rest.
B1, 2, 3, 4, 5. The same as above, except that negative judgments are considered instead of positive ones.

Sample Record Sheet

Observer Miss Noteware. March 24. Round 40, K3.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
A											—				
B												+			
C						+									
D														[illegible]	
E		+													
F									—						
G				+											
H			—												
I								+							
J															—
K	—														
L					—										
M													+		
N										+					
O							—								

"+" means "salt"; "—" means "not salt."

The letters at the left are the arbitrary names of the solutions. This was the 40th series, or round, for this observer and the prearranged programme required it to begin with solution K; this was the third time that K had come around to the head of the series, each solution coming first in turn. The numbers at the head of the table give the sequence of the judgments. The record sheet shows what solution and what judgment preceded any given case.

SAMPLE TALLY SHEET

Observer Mr. Levy. Weak Solutions. Solution D = 0.25%

Positive judgments on D preceded by: (left) — Negative judgments on D preceded by: (right)

		—	—	Q	+	+				
		—	+	O	+	—	—	—	—	—
—	—	—	—	F	—	—				
	—	+	—	K	—	—	—			
		+	—	A	—	—	—			
			+	N	+					
			—	G	—					
+	+	+	—	C	+	—	—	+	+	—
		—	—	J	—	+	+	+		
				B	—	+	+			
				(D)						
+	+	—	—	L	—	+	+			
		+	—	M	+	+				
	+	—	+	H	+	+	+			
	—	—	—	E	+	+				
		0	0	Nothing		0	0	0	0	

A positive (+) judgment on solution D was preceded by:

Positive judgments on solutions weaker than D	7 times
Positive judgments on solutions stronger than D	5 times
Negative judgments on solutions weaker than D	14 times
Negative judgments on solutions stronger than D	7 times
Nothing, i.e., this solution was the first in the series	2 times

A negative (—) judgment on solution D was preceded by:

Positive judgments on solutions weaker than D	12 times
Positive judgments on solutions stronger than D	9 times
Negative judgments on solutions weaker than D	19 times
Negative judgments on solutions stronger than D	1 time
Notning, i.e., this solution was the first in the series	4 times

The solutions are arranged in order of intensity from the top down to the strongest. On the left the table shows what happened each time just before solution D was called *salt*. On the right, what happened just before solution D was called *not salt*. Thus solution L was given 4 times before solution D was called *salt;* in 2 of these cases L was called *salt itself,* anᴗ in 2 *not*. L was given 3 times just before D was called *not salt;* on 2 of these 3 occasions L was itself *called salt,* the other time *not*. Six times D came nrst, so that nothing came before it; on 2 of these occasions it was called *salt,* the other 4 times *not*. Like all the other solutions in this series D was judged 80 times; 35 times it was called *salt* and 45 times *not*. See Table XI, solution 0.25 per cent, for the final tabulation of the data from this tally sheet.

Tables VIII to XII show the results obtained from each person for each solution. In Table XIII there is a statement of the final results with regard to the positive judgments for each person. The results for negative judgments are merely complementary to these.

The general method here employed is to find the total number of positive judgments in any one of the above categories and then to find the proportion of them which had one kind of antecedent and the proportion of them which had another kind. The result is expressed in per cent, and it thus becomes possible to measure the effect of the antecedents upon the judgment. This method has already been employed in ascertaining the fact mentioned on page 217—that all of the observers, but some more distinctly than others, show a tendency to call a solution salt more

TABLE VIII

OBSERVER KOHLBERG, 80 ROUNDS OF STRONG SOLUTIONS

"strong" = stronger than the one in question
"+" = positive judgment of salt

	+ jdmt. preceded by:					jdmt. preceded by:				
Sol. in % NaCl	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.	+ weak	+ strong	weak	strong	0 jdmt. 0 sol.
0.0%	0	2	0	0	0	0	38	0	35	5
0.1	0	0	0	1	0	0	39	0	32	5
0.2	0	4	0	0	0	0	49	3	19	5
0.3	0	3	3	1	1	0	43	6	19	4
0.4	0	6	2	4	2	1	32	12	18	3
0.5	1	7	6	3	0	2	40	9	7	5
0.6	1	21	11	1	2	0	26	9	5	4
0.7	4	10	27	2	0	2	17	9	4	5
0.8	5	13	28	0	0	1	15	11	3	4
0.9	7	17	36	1	4	0	6	8	0	1
1.0	11	17	35	2	3	1	6	3	0	2
1.1	19	14	33	1	5	2	3	3	0	0
1.2	20	15	38	0	4	0	0	2	0	1
1.3	30	7	35	0	4	1	1	1	0	1
1.4	36	5	33	0	3	1	0	0	0	2
1.5	43	0	32	0	4	0	0	0	0	1
Total	177	141	319	16	32	11	315	79	142	48

TABLE IX

OBSERVER VON DER NIENBURG. 40 ROUNDS OF STRONG SOLUTIONS

"strong" = stronger than the one in question
"+" = positive judgment of salt

	+ jdmt. preceded by:					— jdmt. preceded by:				
Sol. in % NaCl	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.
0.0%	0	14	0	0	3	0	23	0	0	0
0.1	0	14	0	1	2	0	21	0	1	1
0.2	1	17	1	0	2	0	17	0	1	1
0.3	2	24	3	0	3	0	5	3	0	0
0.4	7	26	3	0	2	0	2	0	0	0
0.5	4	30	3	0	2	0	0	1	0	0
0.6	10	25	3	0	2	0	0	0	0	0
0.7	15	17	5	0	2	0	1	0	0	0
0.8	13	19	5	0	3	0	0	0	0	0
0.9	18	13	6	0	3	0	0	0	0	0
1.0	19	12	6	0	3	0	0	0	0	0
1.1	22	11	4	0	3	0	0	0	0	0
1.2	23	10	4	0	3	0	0	0	0	0
1.3	31	3	4	0	2	0	0	0	0	0
1.4	27	0	12	0	1	0	0	0	0	0
1.5	31	0	7	0	2	0	0	0	0	0
Total*	14	125	10	1	14	0	68	4	2	2
Total†	223	235	66	1	38	0	69	4	2	2

* First 6. † All 16.

often if it has been preceded by a weaker than if it has been preceded by a stronger one.[19] In the same way one can tell whether a particular stimulus is more apt to be judged salt if the one before it has been judged salt. That is to say, we may expect to see a positive or a negative after-effect from one stimulus affecting the judgment of the next; and along with such a physiological after-effect there may be a psychological tendency to reverse the preceding judgment or to repeat the preceding judgment. Table XIV presents some of the data upon this point which are included in Table XIII. Table XIV is directly comparable to Table VII on contrast.

[19] The data which were presented in Table VII are included in Table XIII.

TABLE X

OBSERVER VON DER NIENBURG. 100 ROUNDS OF WEAK SOLUTIONS
"strong" = stronger than the one in question
"+" = positive judgment of salt

	+ jdmt. preceded by:					— jdmt. preceded by:				
Sol. in % NaCl	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.
0.0%	0	2	0	7	0	0	15	0	69	7
0.025	0	1	0	5	0	0	15	7	65	7
0.05	0	2	1	2	1	0	21	4	63	6
0.075	1	0	0	0	0	1	22	23	46	7
0.1	0	3	0	2	0	2	13	27	47	6
0.125	0	1	5	2	0	4	18	21	42	7
0.15	0	1	3	6	0	1	15	34	33	7
0.175	3	2	6	7	0	3	10	42	22	5
0.2	0	1	12	2	0	2	10	41	25	7
0.225	1	1	19	5	0	4	11	40	13	6
0.25	5	5	19	5	0	3	11	33	13	6
0.275	6	3	21	2	2	7	11	32	11	5
0.3	5	2	29	0	1	7	4	44	2	6
0.325	7	2	38	2	1	5	0	36	2	7
0.35	15	0	44	0	0	10	0	25	0	6
Total	43	26	197	47	5	49	176	409	453	95

In the cases of observers Levy and Noteware we see a tendency to reverse the judgments from one time to the next (Table XIV). Any particular judgment is influenced by the strength of the preceding stimulus, and also it is influenced by the *expression* of the preceding judgment. If the last solution was called salt the present one is not so likely to be called salt, regardless of the actual intensities of the stimuli. Without fully realizing it themselves, both of these observers were doing a good deal of rather blind guessing and each of them was calling any particular solution salt almost as often as not (see Plate 3, figs. 2 and 3.) They seem to have been laboring under the impression (for which no justification could be offered) that about half of the solutions ought to taste salt. Such being their attitude, it is reflected in this tendency to keep things even by alternating positive and negative judgments.

TABLE XI

OBSERVER LEVY. 80 ROUNDS OF WEAK SOLUTIONS

"strong" = stronger than the one in question
"+" = positive judgment of salt

	+ jdmt. preceded by:					— jdmt. preceded by:				
Sol. in % NaCl	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.
0.0%	0	15	0	20	0	0	19	0	21	5
0.025	2	9	0	9	3	3	31	2	19	2
0.05	1	5	5	11	2	1	24	6	22	3
0.075	3	5	1	19	3	6	19	8	14	2
0.1	4	4	3	6	1	5	17	15	20	5
0.125	3	14	14	12	2	4	15	6	7	3
0.15	6	9	7	12	2	5	15	11	10	3
0.175	6	10	9	18	4	4	9	10	8	2
0.2	6	6	17	6	1	13	10	9	8	4
0.225	13	7	10	9	2	10	3	12	10	4
0.25	7	5	14	7	2	12	9	19	1	4
0.275	10	5	13	3	1	14	5	19	6	4
0.3	15	5	24	2	1	9	3	15	2	4
0.325	17	3	22	3	2	8	1	21	0	3
0.35	10	0	32	0	1	14	0	18	0	5
Total	103	102	171	137	27	108	180	171	148	53

Observer Kohlberg (Table XIII) shows a contrast effect between the stimuli, like that of the others; that is, the probability of a positive judgment is increased if the stimulus just preceding was weaker than the one in question. But there is also another tendency at work, for the contrast effect is not equally marked in all cases. When there is contrast between two solutions, A and B, the observer is apt to attest the contrast by calling B salt if it is really salter than A, or *vice versa*, and this tendency exists without regard to the judgment passed upon A; but the striking fact appears that this tendency is stronger if the judgment of A was mistaken. Normally 54 per cent of the judgments in this series are positive, but after weaker solutions 85 per cent of them are positive; after weaker solutions which have been called "salt" 94 per cent of the judgments are positive ("salt"), while if the weaker preceding solution was

TABLE XII

OBSERVER NOTEWARE. 40 ROUNDS OF WEAK SOLUTIONS

"strong" = stronger than the one in question
"+" = positive judgment of salt

	+ jdmt. preceded by:					— jdmt. preceded by:				
Sol. in % NaCl	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.	+ weak	+ strong	— weak	— strong	0 jdmt. 0 sol.
0.0%	0	11	0	11	1	0	12	0	4	1
0.025	0	3	0	6	1	2	14	1	12	1
0.05	0	8	4	4	1	3	11	2	5	2
0.075	1	6	3	6	2	0	12	3	6	1
0.1	0	9	4	1	3	4	9	2	8	0
0.125	4	7	7	6	1	3	5	2	4	1
0.15	2	3	7	7	1	5	7	5	1	2
0.175	8	4	4	7	1	2	4	7	2	1
0.2	7	1	9	5	2	1	5	6	3	1
0.225	9	1	6	4	1	5	6	3	3	2
0.25	7	1	7	1	2	5	3	10	2	2
0.275	9	3	10	5	0	5	2	3	1	2
0.3	11	3	5	2	2	7	4	5	1	0
0.325	8	1	14	1	1	6	2	5	0	2
0.35	9	0	11	0	2	10	0	7	0	1
Total	75	61	91	66	21	58	96	61	52	19

called "not salt" only 78 per cent of the following judgments were positive. In other words, the effect of contrast between two stimuli is complicated by a tendency to repeat a judgment just made. This may be called a tendency to "iteration." It should be observed, however, that the combined figures of Table XIV show a general tendency on the part of this observer in the opposite direction, i.e., to give more positive judgments following negative than following positive judgments.

Two separate records are available for observer von der Nienburg in this connection, one for the weak series and one for the strong series of stimuli. She was able to distinguish the salter from the less salt solutions very clearly in both series. In both series she shows the influence of contrast; more markedly in the strong series, where more than half of the solutions were clearly salt. At the same time the tendency to iteration is seen to be

TABLE XIII. DEDUCED FROM TABLES VIII–XII

Showing what proportion of the judgments are positive when the stimuli have been preceded by stronger or by weaker stimuli which have been called either "salt" or "not salt." The numerator indicates the number of positive judgments; the denominator the whole number of judgments, both positive and negative, which were preceded by stimuli and judgments as indicated.

	Miss K., 80 rounds of 16 strong solutions		Miss v. d. N. 40 rounds of 16 strong solutions		Miss v. d. N. 40 rounds (1st 6 of the above 16)		Miss v. d. N. 100 rounds of 15 weak solutions		Mr. L., 80 rounds of 15 weak solutions		Miss N., 40 rounds of 15 weak solutions	
	Ratio	%	Ratio	%	Ratio	%	Ratio	%	Ratio	%	Ratio	%
Preceding solution *stronger* and called "salt."	$\frac{141}{456}$	=30.9	$\frac{235}{304}$	=77.4	$\frac{125}{193}$	=64.7	$\frac{26}{202}$	=12.9	$\frac{102}{282}$	=36.2	$\frac{61}{157}$	=39.5
Preceding solution *stronger* and called "not salt."	$\frac{16}{158}$	=10.1	$\frac{1}{3}$	=33.3	$\frac{1}{3}$	=33.3	$\frac{47}{500}$	= 9.4	$\frac{137}{285}$	=48.1	$\frac{66}{118}$	=55.9
Preceding solution *stronger* disregarding the judgment.	$\frac{157}{614}$	=25.6	$\frac{236}{307}$	=76.9	$\frac{126}{196}$	=64.2	$\frac{73}{702}$	=10.4	$\frac{239}{567}$	=42.2	$\frac{127}{275}$	=46.2
Preceding solution *weaker* and called "salt."	$\frac{177}{188}$	=94.2	$\frac{223}{223}$	=100.0	$\frac{14}{14}$	=100.0	$\frac{43}{92}$	=46.7	$\frac{103}{211}$	=48.8	$\frac{75}{133}$	=56.4
Preceding solution *weaker* and called "not salt."	$\frac{319}{398}$	=78.2	$\frac{66}{70}$	=94.3	$\frac{10}{14}$	=71.4	$\frac{197}{606}$	=32.5	$\frac{171}{342}$	=50.0	$\frac{91}{152}$	=59.9
Preceding solution *weaker* disregarding the judgment.	$\frac{496}{586}$	=84.6	$\frac{289}{293}$	=98.6	$\frac{24}{28}$	=85.8	$\frac{240}{698}$	=34.5	$\frac{274}{553}$	=49.5	$\frac{166}{285}$	=58.2
Preceding solution called "salt"disregarding the strength.	$\frac{318}{644}$	=49.4	$\frac{458}{527}$	=87.0	$\frac{139}{207}$	=67.1	$\frac{69}{294}$	=23.5	$\frac{205}{493}$	=43.4	$\frac{136}{290}$	=47.0
Preceding solution called "not salt," disregarding the strength.	$\frac{335}{556}$	=58.6	$\frac{67}{73}$	=91.7	$\frac{11}{17}$	=64.7	$\frac{244}{1106}$	=22.1	$\frac{308}{627}$	=49.1	$\frac{157}{270}$	=58.2
Preceding solution: All, *strong* and *weak*, "salt" and "not salt."	$\frac{653}{1200}$	=54.4	$\frac{525}{600}$	=87.5	$\frac{150}{224}$	=67.0	$\frac{313}{1400}$	=22.4	$\frac{513}{1120}$	=45.8	$\frac{293}{560}$	=52.3

TABLE XIV

ITERATION AND REVERSAL OF JUDGMENT. INFLUENCE OF FORM OF THE PRECEDING JUDGMENT UPON THE NEXT JUDGMENT

The last column of percentages shows the proportion of positive judgments from all the cases, without regard to the preceding stimuli or judgments. The next to the last column shows the proportion of positive judgments when the stimulus in question has been preceded by a positive or by a negative judgment.

Miss K., 80 rounds	318	pos. jdmts.	follow	644 pos.	jdmts.,	or, 49.4%	as against a normal of 54.4% positive
of 16 strong solutions	335	"	"	556 neg.	"	or, 58.6%	judgments
Miss v. d. N., 40 rounds	458	"	"	527 pos.	"	or, 87.0%	as against a normal of 87.5% positive
of 16 strong solutions	67	"	"	73 neg.	"	or, 91.7%	judgments
Miss v. d. N., 40 rounds	139	"	"	207 pos.	"	or, 67.1%	as against a normal of 67.0% positive
the 1st 6 of the above 16	11	"	"	17 neg.	"	or, 64.7%	judgments
Miss v. d. N., 100 rounds	69	"	"	294 pos.	"	or, 23.5%	as against a normal of 22.4% positive
of 15 weak solutions	244	"	"	1106 neg.	"	or, 22.1%	judgments
Mr. L., 80 rounds	205	"	"	493 pos.	"	or, 43.4%	as against a normal of 45.8% positive
of 15 weak solutions	308	"	"	627 neg.	"	or, 49.1%	judgments
Miss N., 40 rounds	136	"	"	290 pos.	"	or, 47.0%	as against a normal of 52.3% positive
of 15 weak solutions	157	"	"	270 neg.	"	or, 58.2%	judgments

very strong. The number of positive judgments after a weak solution, or of negative judgments after a strong solution, already relatively large as the result of contrast, is greatly increased if the judgment to be made is a repetition of the one just made. In this observer the tendency to iteration is at work when the solutions are so strong that most of them taste distinctly salt, and it is no less evident when the solutions are so weak that very few of them appear salt.

Thus it appears that for the two persons who were capable of fairly reliable observations a combination of circumstances in which a negative after-effect is reinforced by the fact that a positive judgment has just been made is peculiarly likely to elicit a positive judgment upon the next stimulus. In any event it is clear that the comparatively simple judgment involved in perceiving a weak stimulus is influenced by factors which lie wholly within the individual who is making the judgment. Whether or not a sensation will appear as the result of any particular stimulation depends upon the intensity of the stimulus and the state of attention; but it also depends upon other factors of a very different order. One of these factors, and an important one, is the tendency to repeat the experience which has just been had. Even if the experience itself was illusory, it paves the way for another experience of the same kind. Not even the most elementary sense-experience appears in isolation. Its appearance is governed to some extent by the mental attitudes which precede and usher it into consciousness.

B. The Influence of Surrounding Stimuli; A Central Factor which Dislocates the Threshold of Sensation

The determination of the threshold is subject to another disturbing central factor. Compare the records for observers Levy and von der Nienburg on the weak series (Plate 3.) Both of them give more than 50 per cent of positive judgments on the two strongest solutions, but this fact does not have the same

significance in the two cases. Mr. Levy is habitually free with his positive judgments, so that there are 45 per cent positive even in the case of pure water. In his case, therefor, the 50 per cent point is at the very beginning of things. On the other hand, Miss von der Nienburg is always chary, in this series, of positive judgments, and when she gives as many as fifty out of a hundred we may be sure that she has a large number of clear sensations, for she does not often report salt unless there is some there. But if we turn to the subsequent record of this same observer (v.d.N.) with the stronger stimuli, we find that she also can be prodigal of positive judgments under the influence of a relatively large number of positive sensations (Plate 2.) In the weak series, when the stimulus was water, she gave only nine positive judgments of salt out of one hundred; and for the solution of 0.1 per cent she gave only five; in the strong series she gave seventeen out of a possible forty positive answers for both water and the solution of 0.1 per cent. In the weak series the 50 per cent threshold would fall on a solution of 0.325 per cent and in the strong series it would fall on slightly under 0.2 per cent. Or, stating the same relations somewhat differently, the 50 per cent threshold stimulus of the weak series gives almost 80 per cent positive judgments in the strong series. It is, of course, out of the question to suppose that the sensitivity of the observer is appreciably different in the two series. On the contrary the greater number of positive judgments on weak stimuli in the strong series must be ascribed to the inertia of judgment by which this observer is led to give an undue proportion of judgments of the kind that are already predominant in the series.[20]

Let us see how the matter stands for observer Noteware, who was the only other one taking part in both strong and weak

[20] That this phenomenon does not depend upon special conditions of time and place may be seen from an inspection of some of the data long since reported by Camerer (*Ztsch. f. Biol.*, vol. 6, 1870, p. 443). In one of his endeavors to reach a threshold, using a 7 millimeter glass tube to apply the solution, he tried weaker and weaker solutions on successive days with the peculiar result that the weaker solutions of one day gave as many positive answers as the stronger ones of the day before. By

series. In the strong series she gave, out of forty cases:

for water	0.0%	3	positive "salt" answers
for solution	0.1%	8	
	0.2%	1	
	0.3%	1	

But in the weak series she gave, again out of forty cases:

for water	0.0%	23	positive answers
for solution	0.1%	17	
	0.2%	23	
	0.3%	23	

In the strong series, which came first with her, the 50 per cent threshold stimulus is stronger than a solution of 0.6 per cent. This is considerably stronger than the strongest solution to be

---

condensing his table we get the following:

1st day		2nd day		3d day		4th day	
% salt	% pos. jdmt.	% salt	% pos jdmt.	% salt	% pos. jdmt.	% salt	% pos jdmt.
0.39	69						
0.31	61						
0.25	31	0.25	71				
0.00	23	0.22	66				
		0.19	32				
		0.00	27	0.14	64		
				0.12	49		
				0.11	28	0.11	32
				0.00	15	0.10	25
						0.09	8
						0.00	11

From this it appears that the solution of 0.25 per cent, which was the strongest on the second day, gave as many positive judgments as were given by the solution of 0.39 per cent which was the strongest of the preceding day, and gave more than twice as many positive judgments as it had itself given on the preceding day when it was the weakest in the series. The phenomenon continues very strong for three days, disappearing on the fourth; but on a later occasion a solution as weak as the weakest in the series of the fourth day gave 79 per cent of right answers for the same two observers, or more than even the strongest solution of the previous days. It should be noted that the ability to detect water remains about the same from day to day, so that the increasing number of positive judgments on the weaker solutions really means a keener power of discrimination between water and salt. Camerer himself attributes this phenomenon in part to practice, but in part to "unconscious comparison" between the various solutions tasted on any one day.

For these two observers the "absolute threshold" depends very largely upon the strength of the solutions entering into the series upon which the tests are made. The threshold is lower in a group of weak solutions than it is in a group of stronger solutions.

found in the weak series, and yet when she comes to the weak series she gives 50 per cent or more of positive judgments on ten out of the fifteen solutions. It will be observed that these data are not in agreement with those from observer von der Nienburg. The latter gave more positive judgments and had a lower nominal threshold in the strong series. But observer Noteware gives more positive judgments and has a lower nominal threshold in the weak series. In this case we know that all the solutions in the weak series are well below what seems, by a fair determination, the threshold in the strong series. The subject herself felt that she was doing little more than guessing in the weak series, and a mere glance at the graphic representation of the data (Plate 3, fig. 3) shows that her feeling was justified. She was actually working with intensities which were, in one sense, below the threshold. Yet she could give more than fifty positive judgments to the hundred. It should be noted also in this connection that she gave more positive judgments on the seven strongest solutions than she did on the seven weakest, so that there is still a threshold of difference represented within the series. But as a consequence of the disconcerting nature of the situation she fell back upon guessing, and as a guide in her guessing she seems to have been inclined to make the supposition that about half of the solutions contained salt and the other half not. There are 314 positive judgments out of the possible 600.

It is apparent in a case like this that the mere fact that some stimulus is called salt more often than not salt is in itself no proof that the stimulus in question is above the threshold of sensation. Other factors may be responsible for the preponderance of positive judgments. Judgments must be accounted for psychologically as well as psychophysically, and one of the most important of the psychological conditions which must be considered is the magnitude of the other stimuli which are subject to judgment in the same series. The discovery of the importance of this particular factor was one of the most conspicuous results of the writer's investigation of the judgment of difference (page 51 of this volume).

It appears from what has just been said that the observers who worked with both strong and weak solutions maintained different attitudes in the two series, showing that their judgments were affected by the total group of stimuli so as to be disturbed with regard to any member of one group that might be transferred to the other group. This is best seen in the case of observer von der Nienburg, who increased the number of positive judgments upon weak stimuli when she was making a good many positive judgments in the series.[21]

Observer Noteware seems to have been seriously influenced by the general proportion of the judgments. She gave always about as many positive as negative judgments; so that, when she came to judge very weak stimuli, she gave several times as many positive judgments upon certain ones as she had given upon the same stimuli when found among stronger solutions. Some place had to be found for what she felt to be the proper proportion of positive judgments, and they fell by chance all along the scale, piling up where otherwise there would have been very few of them.

During the course of the experiment, all of the observers were, from day to day and more or less steadily, inclined to change their opinion as to the number of salt solutions in a series. What the grounds of this change were does not appear. Perhaps there was merely a shift from a comparatively unfounded prejudice to an opinion better in accord with the actual sensations.

Table XV shows the percentage of positive judgments made by each of four observers at each successive sitting during the course of the experiment. Observer Levy shows a good deal of irregular fluctuation; on some days there were more positive judgments, on others less. Such results might be due to fluctuations in the condition of the observer, to more local conditions in the mouth, or to the weather. Observer Noteware gives about

[21] This tendency is associated with the tendency to repeat a preceding judgment, which has been discussed already in a previous paragraph. When once it is found that one type of judgment is more frequent than the other type in a series, the tendency to iteration will result in a still further increase in the number of judgments of the predominant type.

TABLE XV

CHANGE OF ATTITUDE FROM SITTING TO SITTING

Showing the average number of positive judgments per round of 15 or 16 stimuli given at each sitting.

Sitting	Miss Kohlberg 16 strong stimuli	Miss von der Nienburg 16 strong stimuli	Miss von der Nienburg 15 weak stimuli	Mr. Levy 15 weak solutions	Miss Noteware 15 weak solutions
1	11.5	10.6	12.8*	6.3	6.4
2	9.4	14.4	7.8*	5.3	8.0
3	9.9	13.7	7.9*	7.4	7.0
4	7.8	14.9	3.66	6.7	9.5
5	8.2	14.6	4.88	8.2	8.0
6	8.3		4.00	6.6	....
7	8.9		3.27	7.0	....
8	8.0		2.39	6.8	....
9	7.9		1.50	6.3	....
10	7.3		2.28	6.8	....
11	9.4		2.00	7.4	....
12	7.8		2.25	6.7	....
13			3.38	7.1	....
14			3.38	6.0	....
15			4.00	....	....
16			3.63		....
Average	8.6	13.7	3.18	6.75	7.85

* Not included in the average.

the same number of positive judgments each day. The result proved that neither of these two observers could really discriminate between the solutions. The number of positive judgments represents merely the attitude assumed by the observer as the basis for guessing. Each of them assumed that about half of the solutions were salt and gave about fifty per cent of positive judgments each day.

Observers Kohlberg and von der Nienburg were not guessing to any great extent. Miss Kohlberg changed her attitude some-

what irregularly from day to day, but in general she gave fewer positive answers as the work progressed from week to week. Miss von der Nienburg gave many more positive judgments in the preliminary experiments with the weak series of solutions than she did later on. The table shows, in the column for weak stimuli, that, after the third day, there was no further regular shift in the attitude of this observer. The attitude finally assumed required about one positive judgment to four negative judgments. There was a larger proportion of positive judgments on some days and a smaller proportion on others, but there was no further systematic change during the course of the experiment.

When observer von der Nienburg came to take up the series of stronger solutions, her attitude underwent a change in this, as in other respects. In this series she gave, on the average, more than four positive judgments to one negative judgment, and there was a tendency to increase the proportion of positive judgments after the first day. From the data it is clear that this observer required two or three sittings before becoming adjusted to the conditions of the experiment, or before becoming adjusted to changed conditions, but that when once she had assumed an attitude toward either series she showed no disposition to disturb the normal ratio between the numbers of positive and negative judgments.

These shifts in the general proportion of positive and negative judgments are significant because they involve a corresponding shift in the apparent position of the threshold. When there is a larger total number of positive judgments in the series, there will be more of them on each member of the series, and the result will be that a solution will appear to be above the threshold, whereas the same solution would appear to be below the threshold if the total number of positive judgments in the series, and consequently on that solution, were reduced.

V

THE INTERPRETATION OF THE THRESHOLD OF SENSATION IN VIEW OF THE DISTURBING CENTRAL FACTORS

There are at least two ways of defining intelligibly the threshold of sensation. The first, which may be called the physiological, regards it as an intensity of stimulus so weak that any weaker stimulus of the same kind would produce no sensation or reaction at all.[22]

Obviously there are stimuli so weak that they are never apprehended by the sense-organs, but it does not follow that there is any discoverable and measurable stimulus which is just strong enough to be apprehended. Certainly an inspection of typical curves of the distribution of judgments such as we have in the accompanying plates gives no warrant for asserting that we can ever find a definite stimulus just strong enough to provoke a positive judgment. The first impression produced by these curves is that every stimulus, no matter how weak, is capable of producing a sensation, but that the stronger ones are more apt to do so than the weak ones. But there are numerous details to be considered before finally adapting this interpretation of the curves.

One of the peculiar things about the curves (see Plates 1 and 2) is that some of them start from a comparatively elevated plateau instead of starting off low when the stimulus was pure water and thence increasing steadily the number of positive judgments. It will be seen that most of the curves which are otherwise regular do proceed in a regular staircase fashion from the very beginning. But the exceptions are worthy of consideration. The record of

---

[22] A direct statement of this point of view may be found in Titchener, *Experimental Psychology*: Student's Manual, Quantitative, Introduction, p. xxxvii. The general assumption that "a stimulus must attain a certain magnitude in order to arouse a sensation at all" is so universally made that other authorities need not be cited.

observer Bates (Plate 1) is typical in this regard. While they are regular enough so far as the stronger solutions are concerned, there is a plateau representing the judgments on the five weakest solutions. Each of these solutions, including water and the strongest of them being 0.4 per cent, is called salt in 25 per cent or more of the cases. In the case of observer Whisman (Plate 2) the plateau is not elevated; instead there is no rise whatever in the curve until a solution of 0.4 per cent is reached.

In the first of these cases it may be that the taste of the water is strong and that it is confused with the taste of salt. As the dose of salt is increased it does not increase the taste of the water but does gradually submerge it. So, according to this strictly hypothetical explanation, the first few solutions would all have a taste, interpreted as salt, but really a mixture of salt and the water taste, and all these solutions would have about the same taste until the salt became strong enough to be tasted alone, over and above the taste of the water. Unfortunately no record was preserved of this observer's separate estimate of the taste of the water. It may be presumed that the whole record of observers Levy and Noteware with the weak solutions (Plate 3) is nothing more than the nearer view of such a plateau at the beginning of the curve before any true positive cases begin to appear. Observer Noteware shows a tendency to form such a plateau even in the stronger series (i.e., in Plate 2) and shows also, as many others do—the clearest example is seen in the weak series for observer von der Nienburg (Plate 3)—a slight tendency to confuse the taste of water with salt, and to find a very weak salt solution more tasteless than water.[23]

A somewhat similar hypothesis may be advanced with more certainty to account for the plateau at the base line in the case of observer Whisman. This observer was familiar with the disagreeable taste of the water and frequently said "that's water" with an emphasis that denoted more than the mere absence of salt. Now since the water is clearly recognized for itself, it obscures the salt taste until that becomes strong enough to be

[23] For a discussion of the taste of water see Appendix I.

detected over the water. In other words, we seem here to be dealing with a case of discrimination between nearly complementary tastes; and in the four weak solutions the taste of water is recognizable each time, then it is not recognized so often, next there is a sudden leap to cases where salt is recognized in a majority of the cases, and finally there are three solutions in which the salt is recognized every time. It is conceivable that similar results might have been obtained from other observers if they had been equally familiar with the water taste and had differentiated it from, instead of confusing it with, the salt taste.

On the whole, the records which are the most consistent within themselves, and which seem to have been taken under the most favorable circumstances so that there are few stimuli which are unnecessarily strong and yet enough strong ones to give a fairly large number of clear positive judgments, show regular increases in the number of positive judgments, beginning with water or the most tasteless mixture of water and salt. Thus the record of observer von der Nienburg for one hundred rounds of the weak series (Plate 3) shows only one positive judgment for a solution of 0.075 per cent (the weaker solutions had a stronger taste arising from the water, or at any rate, were more often called salt), but from that point on each increase of one quarter of one per cent in the strength of the solution increases the number of positive judgments with almost perfect regularity. In this case it seems certain that every intensity of stimulus employed has produced at times a salt sensation with the exception of the 0.075 per cent solution which is nearly neutral, and the weaker ones, which, while they seemed to taste salt, probably produced a somewhat different sensation. Data of the same type are given by observers Robinson and Noteware for the stronger stimuli. Other examples of what appears to me to be the normal situation are found in the records of observers Kohlberg, Jackson, and Libbin. All of them show a quite regular stepping-up from the weakest solution to the strongest. Several other instances show the same regular stepping-up but involve a considerable number of positive judgments on even the weakest solutions;

for example, observers von der Nienburg (strong series), Detter and Ham. Whatever the reason for the excess of positive judgments on the weakest solutions, it can not be adduced to prove that it is possible to measure the weakest solution which can be tasted. The easiest explanation of the positive judgments on water and weak solutions would be that the water tasted salt, but this hypothesis is inconsistent with all the direct observations, and it would not hold for observer von der Nienburg. A more psychological explanation has already been advanced in her case, namely, that the large preponderance of distinctly salt solutions created a tendency to call a large proportion of all of the solutions salt.

All of the data obtained have now been reviewed. Of the thirteen records, two cases (Bates, Whisman) seem to indicate the existence of intensities less than the weakest which can be perceived, but there is evidence that this interpretation is not the only one open; two cases (Levy, Noteware, weak stimuli), are badly confused by the absence of clear salt sensations in the series, and are capable of explanation either as cases of unperceived stimuli or in another way; six (Libbin, Jackson, Kohlberg; von der Nienburg, strong; Detter, Ham), show no trace of an unperceived intensity; three (von der Nienburg, weak; Noteware, strong; and Robinson), show a saline solution or two which is not perceived as salt, but do not indicate that there is a class of solutions too weak to be perceived.

The distributions of judgments reported by Camerer[24]—the only ones hitherto made which are at all comparable with the present work in respect of the number and intensity of the stimuli employed—do not show any imperceptible intensities. The data from three of Camerer's four observers are presented in Plate 4 in as good a graphic form as I can devise for them. The fourth observer shows no characteristics not found in one of these three. Only the results of the method "without knowledge" are considered. Each observer made fifty rounds of judgments on the set of nine solutions. The unit solution was only 0.0159 per cent

[24] *Ztschr. f. Biologie,* vol. 21 (1885), p. 578.

and the weakest was only one-eighth as strong as the unit. It will be seen that although these intensities are much less than those employed by me (but the quantities administered are ten times as great), the curves both for number of right judgments and for right judgments plus half the number of uncertain judgments, begin to rise from the weakest solutions and rise gradually as the solutions become stronger. Even the weakest solutions (0.002 per cent and 0.004 per cent) are not exactly equivalent to each other or to water. With regard to the absolute intensities here, allowance should be made for the fact that Camerer worked with "Brunnenwasser," which contained an unspecified but appreciable quantity of lime. The fact remains, however, that very minute increments of salt did influence the number of positive judgments on this water.

A second view of the nature of the threshold seeks to place it at some point on a scale midway between the intensity which is just barely strong enough to produce a sensation and the intensity which always produces a sensation. From the nature of the disturbances of judgment by the central factors discussed in the last few pages, it should be clear that the threshold can not be located by arbitrarily deciding *a priori* that a stimulus above the threshold ought to be perceived in more than half or more than three quarters of the cases. Whether a given stimulus is perceived in half or in three quarters of the cases depends too much upon irrelevant accidents like the presence of too few or too many strong stimuli in the series in question. It should be noted particularly that the 50 per cent and 75 per cent thresholds here have a different meaning from that which they have in the case of the difference threshold; for when one is comparing two intensities his judgment is as apt, by mere chance, to fall one way as the other, but there is no such easy law when one is looking out for a sensation. One might, indeed, suppose that the series had been so prepared that half of the solutions would taste salt and half not, and in that case the chances would be even that any particular solution would or would not taste salt. But our observers were cautioned that no such arrangement was

contemplated, and that the differences between individuals would make such an arrangement impossible.

In the case of the difference-threshold 50 per cent of right judgments represents the minimum of discrimination, but in the case of the absolute threshold 50 per cent means pure hit-and-miss for one person (the one who merely guesses, upon the supposition that about half will taste salt), while for another person 50 per cent means a high degree of assurance compared with the 10 per cent or 20 per cent of positive judgments accorded to weaker stimuli.

VI

A MEASURE OF SENSITIVITY PROPOSED AS A SUBSTITUTE FOR THE CONVENTIONAL THRESHOLD

Is there then any less arbitrary method of establishing a threshold of sensation anywhere between the stimulus which is always perceived and the one which is never perceived? The way to answer this question in the affirmative is much simpler than I, for one, had any expectation of finding it. It appears from the observations in this case, as generally, that when the intensities in a series advance by arithmetical (equal) steps, the number of positive judgments advances at first slowly, with very weak stimuli, then more rapidly, and finally, with strong stimuli, more slowly again until it stops entirely. In the conventional way, used in this paper, of representing the steps graphically, the steps are all of one width, but they are highest in the middle and become shorter at both top and bottom of the stairs. This is all in accord with Weber's Law, and may indeed be in accord with Fechner if it can be shown that the magnitudes of the steps follow a logarithmic series.

In such a graphic representation one step in the middle of the curve is higher than any of the others and marks the point where the curve turns from concave to convex. Without precluding in the least a more exact determination of this critical point by the application of the calculus we may approximate it graphically. Once it has been discovered, it proves to be an admirable substitute for the conventional threshold.

This point can be defined precisely and has exactly the same significance for every person and for every kind of stimulus. It corresponds to the intensity of stimulus from which a change of a given amount will be most frequently or easily recognized.

This point can be determined in a comparatively small number of trials, provided the series contains enough different intensities, embracing both extremes of the scale and arranged in an arithmetical progression.

The peculiar advantage of this measure of sensitivity is that it is independent of the constant proportion between the positive and negative answers, and also independent of the errors which accumulate near the extremes of the scale as the result of imperfect attention. Thus, an observer may double the total number of positive judgments all along the scale as the result of some of the disturbing factors which we have found in actual operation, without affecting in the least this crucial point. So, too, one may give very inconsistent judgments upon very weak or very strong stimuli, and still it will remain possible to determine accurately the threshold according to this method.

Mere inspection of the graphic representation of the data will often suffice to determine such a crucial point; for example, the curve for observer Whisman (Plate 2) gives an undoubted threshold between 0.7 per cent and 0.8 per cent, the steps above and below this step are much smaller, and this single step reaches from 25 per cent of correct judgments to 77 per cent; the 0.7 per cent solution and all the weaker ones are very weak or subliminal (only rarely recognized); the 0.8 per cent solution and all the stronger ones are very strong (almost always recognized). For observer von der Nienburg (strong solutions, Plate 3) the crucial point lies between the 0.2 per cent and 0.3 per cent, while for the same observer when using the weak stimuli (Plate 2) it may be placed between 0.3 per cent and the next stronger solution, but here the data are not adequate. For observer Jackson (Plate 2) it may be placed between 0.7 per cent and 0.8 per cent. For observer Detter (Plate 1) between 0.3 per cent and 0.4 per cent, and for observer Noteware (strong series, Plate 2) between 0.5 per cent and 0.6 per cent. In these latter cases the crucial point can safely be placed at the apparent center of the group of largest steps, even though the particular step at the center happens to be comparatively small. It may be

seen at once, however, that these determinations are only approximate. No great accuracy can be expected from work of this sort carried out by novices and continued only for a few sittings (the forty-round series usually required only five sittings). All that is claimed for this crucial point is that, with due regard for the validity of the data from which it is obtained, it has psychological significance as a substitute for the conventional threshold.

TABLE XVI

ARRAY OF STEPS FOR DIFFERENT OBSERVERS

Showing the increase in the number of positive judgments resulting from an increase of 0.1 per cent in stimulus intensity.

Steps	Observer: Bates	Detter	Ham	Jackson	Kohlberg	Libbin	Noteware	Robinson	von der Nienburg	Whisman
0.0–0.1%	0	2	–2	–1	–1	5	5	–5	0	0
0.1–0.2	–1	3	5	1	2	–1	–7	0	4	0
0.2–0.3	–1	7	7	2	3	3	0	18[4]	11[4]	0
0.3–0.4	0	6[2]	6[2]	2	4	2	3	4	6	1
0.4–0.5	9[1]	7	7	1	5	7	11	5	1	4
0.5–0.6	–3	5	2	6	8	2	4[2]	2	1	5
0.6–0.7	4	0	–2	11	4	4[3]	14	6	–1	0[5]
0.7–0.8	5[1]	–2	3	–1[2]	–1	–1	5	–1	1	21
0.8–0.9	4	2	0	13	7	11	0	4	0	5
0.9–1.0	3	2	–1	3	1	2	1	0	0	3
1.0–1.1	–1	–1	2	2	5	3	1	0	0	–2
1.1–1.2	0	1	–1	0	0	0	–3	0	0	2
1.2–1.3	4	0	1	0	1	0	3	0	0	1
1.3–1.4	3	0	0	0	–2	2	0	0	0	0
1.4–1.5	1	0	0	0	2	0	0	0	0	0

[1] It is assumed that the large step from 0.4 to 0.5 is due to errors. Judging by the number of positive judgments on 0.4 and 0.6 there should not be so many on 0.5.

[2] Intermediate between two large steps.

[3] This case is highly doubtful. There is no safe ground for choosing between the maxima at 0.4–0.5 and at 0.8–0.9. The critical point is assigned to the position half way between the two maxima only because some position is desired as a basis for averaging later.

[4] The steps are too coarse for an accurate determination.

[5] Errors at this stage make the next step extraordinarily large.

In practice we have no need of exceedingly delicate determinations of sensitivity. If such need does arise, however, it can be satisfied only by the accumulation of statistically valid data, not by over-refining on inadequate data; and even in that case it will be found, I am sure, that the simple method here suggested gives a threshold the precision of which is commensurate with the adequacy of the data. For any given number of experimental observations this method has the advantage over the usual methods of determining the threshold, that it is not subject to errors from the inclusion of the observations on the strongest and weakest stimuli. As a result of disturbances of attention, the observations on these strong and weak stimuli are not as reliable as are observations upon stimuli of moderate intensities.

In some cases the superficial inspection of the curve is helped out by a perusal of an array of the arithmetical magnitudes of the steps. Such an array is shown in Table XVI for the ten records from the stronger series of salts. It may be seen again that it is much simpler to find the largest step, or the center of the group of largest steps, than it is to find the true zero at either the top or the bottom of one of the columns of figures. It is of some interest further to find how the threshold discovered by

TABLE XVII

Showing the per cent of positive judgments corresponding to the threshold as determined by the largest step in the curve.

Observer	Largest step lies		Per cent positive jdmts.	
Bates	between 0.7%	and 0.8	between 50%	and 63%
Detter	0.3	0.4	50	65
Ham	0.3	0.4	57	73
Jackson	0.7	0.8	57	56
Kohlberg	0.5	0.6	35	55
Libbin	0.6	0.7	47	57
Noteware	0.5	0.6	37	48
Robinson	0.2	0.3	5	50
von der Nienburg	0.2	0.3	52	80
Whisman	0.7	0.8	25	77
Average	0.47	0.57	41.5	62.4

this method is related to the one obtained by the conventional 50 per cent method, which assumes that a stimulus is above the threshold if it is detected in more than half of the cases in which it is presented.

Table XVIII

Showing the average size of the step at the critical point and for intensities above and below the critical point. Compare Plate 3, fig. 4.

	Average position of step	Average number of positive judgments forming step
Third step below the critical one	0.2%–0.3%	2.4 out of 40
Second ” ” ”	0.3 –0.4	2.5 ” 40
First ” ” ”	0.4 –0.5	4.1 ” 40
The critical step itself	0.5 –0.6	8.2 ” 40
First step above the critical one	0.6 –0.7	5.8 ” 40
Second ” ” ”	0.7 –0.8	3.7 ” 40
Third ” ” ”	0.8 –0.9	0.9 ” 40
Fourth ” ” ”	0.9 –1.0	1.3 ” 40
Fifth ” ” ”	1.0 –1.1	1.3 ” 40
Sixth ” ” ”	1.1 –1.2	0.5 ” 40
Seventh ” ” ”	1.2 –1.3	0.7 ” 40

Note.—Since two of the critical points lie at the second and third members of the series, they give no values for the third step below the threshold. Two others would give no values for a fourth step below, and three would give no values for an eighth step above the critical one.

Table XIX

Average number of positive judgments out of 40 given by 10 observers on 16 strong solutions. Compare Plate 2.

Observer	Solution 0	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Bates	12	12	11	10	10	19	16	20	25	29	32	31	31	35	38
Detter	8	10	13	20	26	33	38	38	36	38	40	39	40	40	40
Ham	13	11	16	23	29	36	38	36	39	39	38	40	39	40	40
Jackson	1	0	1	3	5	6	12	23	22	35	38	40	40	40	40
Kohlberg	1	0	2	5	9	14	22	26	25	32	33	38	38	39	37
Libbin	1	6	5	8	10	17	19	23	22	33	35	38	38	38	40
Noteware	3	8	1	1	4	15	19	33	38	38	39	40	37	40	40
Robinson	7	2	2	20	24	29	31	37	36	40	40	40	40	40	40
von der Nienburg	17	17	21	32	38	39	40	39	40	40	40	40	40	40	40
Whisman	0	0	0	0	1	5	10	10	31	36	39	37	39	40	40
Total for ten	63	66	72	122	156	213	245	285	314	360	374	383	382	392	395

Table XVII shows that the conditions under which the observers worked with the strong stimuli were such that the crucial point, as determined by an inspection of the curves, generally lies in the neighborhood of the intensity which yields fifty positive judgments per hundred. Making a crude average of the ten determinations, the threshold is found, for the group, between a solution of 0.47 per cent and one of 0.57 per cent. The former yields on the average 41.5 positive judgments per hundred and the latter 62.4. If the crucial point is placed half way between them it will yield 52 positive judgments per hundred. The agreement between the two modes of determination holds roughly for each individual case in the sense that the step constituting the crucial point either bridges the stimulus which would yield 50 per cent of positive judgments, or else has as one of its limits an intensity which yields very nearly that proportion. The worst departure from the rule is found in the case of observer Ham, for whom the point is found clear above the 57 per cent level. Inspection of the record will show (Plate 1) that there is justification for thinking that the crucial point has been placed too high in this case.

Experiments with weaker stimuli lead to the conclusion that the correspondence just spoken of is the result of the particular arrangement of conditions and is not inherent in the nature of the threshold itself. The determination for observer von der Nienburg with the weaker solutions gives a much lower number of positive judgments in spite of the fact that the solutions selected as marking the crucial step are stronger in this case.

Weak series, crucial step between 0.3% and 0.325% yields between 36% and 50% of positive judgments.

Strong series, crucial step between 0.2% and 0.3% yields between 52% and 80% of positive judgments.

So, too, the data from observers Levy and Noteware indicate that more than half of the judgments may be positive without reaching a threshold.

Before leaving the subject of the threshold it may be worth while to call attention to the results obtained when a whole group of persons is examined in the mass. Combining the separate determinations for the individuals, we obtain from the average a measure of the sensitivity of the group. The crucial point for the group lies between an intensity of 0.47 per cent and one of 0.57 per cent. It is therefore about twice as high as the threshold intensity usually given as determined by the "physiological" method, i.e., the weakest solution which can be tasted at all.[25]

We may also measure the average size of the step determined upon as the crucial one as in Table XVIII. This is found to be 8.2. That is to say, on the average, out of forty possible judgments, the solution just above the crucial point receives eight more positive judgments than the one just below. And further, if we obtain, by averaging, the size of the steps on each side of the crucial point, we see more clearly than in any individual case that the steps decrease in size as the solutions become stronger or weaker than the threshold intensity. This is shown graphically in Plate 3, fig. 4. No particular mathematical validity can be attached to this average, however, because, of course, the steps are of very different values for the different observers and so should not be equally weighted, as they here are; but it seems to support the theory upon which the present method of approximating a threshold is based.

From Table XIX, giving in full the number of positive judgments made by each observer on each solution, it is possible to obtain an average of the number given by all the observers for each solution. A graph representing this average and constructed like the graphs representing the individual records is shown at the bottom of Plate 2. As might be expected, most of the significant features of the individual records are hidden under this average. One can see, however, that each increase in the strength of the salt solution increases the frequency with which the salt is observed. There is only a trace of a crucial

[25] See Appendix II.

point left between 0.4 per cent and 0.5 per cent (the average of the different determinations falls between 0.47 per cent and 0.57 per cent), in the neighborhood of the stimulus which is perceived in half of the cases (the 0.5 per cent solution). The various determinations of the crucial point for different individuals are scattered over the entire middle range of intensities, giving to the average curve the appearance of a series of almost equal steps in this region. Only when we come to the strongest and weakest solutions, which are never liminal for anyone, does it become evident that the size of the steps decreases as we depart from the threshold-intensity in either direction.

VII

CONCLUSION

A systematic attempt to analyze the conditions under which judgments are made in a typical instance in determining an absolute threshold of sensation leads to the conclusion that such a determination is fruitless if conducted along the customary lines. Not only are successive judgments upon weak stimuli subject to contrast-effects in a marked degree, but they are also disturbed by a tendency to repeat or contradict a previous judgment.

Moreover, the judgments are so affected by the general trend of the earlier stimuli and judgments in the series, that by judiciously arranging two sets of stimuli the same person can be made to appear to have a high threshold in one set and a low one in the other set.

In view of these difficulties some plan must be devised for measuring the delicacy of sense-perception in a more reliable way. The graphic representations of the distribution of the judgments of individual persons invites our attention to the fact, long established, that equal increments of stimulus intensity do not carry with them equal increases in the number of positive judgments representing sensation. At some point in the scale of stimulus-intensities a given increase in intensity has the effect of producing a maximum increase in positive judgments. This critical intensity can be discovered with comparative ease, and its determination is not affected by the errors which vitiate the determination of the least perceptible stimulus.

It is suggested that this critical point in the stimulus series be taken as an index of the delicacy of sense perception. It would be idle to talk of abolishing the least perceptible stimulus as a standard index, but we should recognize its inadequacy and should be ready to adopt a more rational and consistent index, not perhaps as a substitute but as an auxiliary, when we are seriously endeavoring to measure accurately the endowment or condition of individuals with regard to the mechanisms of sense-perception.

APPENDIX I

The Taste of Distilled Water, Including an Account of a Direct Experiment upon the Subject

The water used in this experiment, although "pure" according to ordinary standards, was by no means tasteless. All of the observers noted that the water had an undefinable, slightly disagreeable, taste; but none of them would admit that it tasted either salt, sweet, sour, or bitter. Some of them described the water as "flat," others as "musty." An inspection of the curves in the plates reveals the fact that several persons had a tendency to call the water "salt" (unknowingly) more often than some of the weakest salt solutions. In other words, the taste of the water is mistaken for salt but can be compensated for by salt—a rather contradictory state of affairs. Titchener, in his *Experimental Psychology*, vol. 1, pt. 2, p. 101, speaks of neutralizing the water by the addition of a little salt in case it tastes sweet or sour. But he does not think that salt will neutralize a bitter taste in the water, and he does not consider the possibility just broached of neutralizing a salt taste by the addition of salt. At any rate he does not think distilled water ever tastes salt, and in this he is undoubtedly correct, notwithstanding the fact that its taste is sometimes confused with that of salt.

A direct investigation of the taste of the water used in this experiment was instituted, and it reveals the fact that most persons call it bitter rather than anything else. In this investigation a single set of judgments was secured from each of one hundred persons. These observers tasted from a porcelain spoon one cubic centimeter of each of nine solutions in the following order:

1. 2.0% salt	6. 0.0625% salt
2. 1.0% salt	7. 0.0% salt
3. 0.5% salt	8. 0.0% salt
4. 0.25% salt	9. 0.0% salt
5. 0.125% salt	

They were told that they would first taste salt and that the successive solutions would be weaker and weaker, and that they were to report in every case just what they tasted, whether salt or anything else. The experiment purported to discover the least perceptible intensity of salt solution. Aggregating the results for the three doses of water which contained no salt we have the following distribution of judgments:

No taste—"water"	49.3 per cent
Bitter	25.4 per cent
Sweet	8.3 per cent
Unknown taste	7.0 per cent
Other taste (known)	4.0 per cent
Salt	3.0 per cent
Sour	2.7 per cent

It seems certain that this particular water does not taste salt. Making allowance for the large number of university students who are familiar with the water and immediately put it down as "no taste," simply because they have been taught that that is the proper thing to say about distilled water, there can be little question that the water is slightly bitter for most persons.

The experiences of other experimenters help, by their diversity, to explain the difficulties which confronted our observers when they were asked to pass judgment on the taste of the water or of extremely weak solutions of salt in the water. Richards speaks of a double-distilled water as being "unusually tasteless," but so far as I know there is no evidence that additional purity involves a loss of taste; in fact we should expect that if the taste depends either on the chemical constitution of the water or on the absence of oxygen (the two principal lines of explanation) an increase of purity would increase the taste. On the other hand Nagel suggests that the (for him) bitter taste of water may be due to the floating of particles of matter which are already present in the mouth on to new locations where they are tasted. In that case the purity of the water would have nothing to do with its taste, and it is a little difficult to see how stimulation of single papillae by water can produce a taste. But Kiesow as well as

Camerer found that water tasted bitter at points all over the tongue when applied in a punctiform manner.

Kahlenberg is the only authority that I can find willing to assert that "pure water is tasteless." His reasons for thinking it ought to be are good, but I question the accuracy of his observation. Keppler could detect no taste in distilled water, but he discovered a smell, which is worse. Kiesow does not attribute the taste to the water itself but to the mouth. The permanent mouth-taste is generally bitter, he thinks, but there may be a sweet after-effect from salt or sour. Under no circumstances does he discover salt in this mouth-taste.

Richet shares the opinion of many when he remarks that distilled water has "a very disagreeable taste" which it is hard to disregard. On the other hand Largier des Bancels thinks that it is sweetish, although he will admit that it may be *fade*. The latter quality is generally unpleasant, I believe.

Camerer, who devoted his youth and his middle age, if not more of his life, to this subject, has made some very acute observations. Clean snow-water he found to be ideal, but it is not easy to obtain. The apothecary's distilled water smelled of the still. Water from the Danube, even in its mountain reaches, also smelled. Well-water containing lime, tastes a little bitter. The admixture of a little salt to this water makes it taste "soft," while a weak quinine solution resembles the well-water in tasting "hard."

Titchener suggests that the bitter sometimes noted as a contrast effect from sugar or salt may be due to an agreement between water and bitter, in that both taste "smooth." Considering this with Camerer's observation just quoted, we may infer perhaps that the "taste" of water is not, after all, a taste-quality but is due rather to the presence or absence of some accompanying tactual characteristic; the absence, perhaps, of the "bite" which is associated with sweet, salt, and sour alike.

The upshot of all of these observations seems to be that water is not tasteless (in the broader sense of the word), that it tastes more like bitter than anything else, and that its taste can be

neutralized by salt. In any event it does not taste like salt, so that we can be sure that a series of salt solutions can be started from a true zero point with regard to that quality. Furthermore there is evidence that some of the weakest solutions of salt in water give a substance which is as nearly tasteless as any which can be produced.

APPENDIX II

Concerning Various Modes of Applying the Taste Stimulus, and Various Determinations of the Threshold for Salt, with an Experimental Contribution

A large number of independent determinations of the threshold for salt are available, but they have been secured by such divers methods, both with regard to the mode of applying the stimulus and with regard to interpretation of the threshold, that comparisons between them are of doubtful value. In the sense of taste the threshold determinations depend primarily upon the mode of administering the stimulus. The least perceptible stimulus may be stated in term of the concentration or "strength" of the solution employed. But this can be significant only if we know also the amount of the solution taken into the mouth, for only then do we know the absolute amount of the sapid substance brought into contact with the taste organs. Numerous devices have been employed in the endeavor to measure the actual intensity of the stimulus, either in terms of "strength" or of gross amount.

Michelson[26] mixed a little gum-arabic with the solution to keep it from running, and then applied a single drop. The same method was employed by Kiesow and Hahn.[27] This method is designed for local exploration rather than for quantitative measurement.

Zwaardemaker[28] used a solution thickened with gelatin and applied with a glass rod for exploration. For quantitative work he prepared pellets of Hollundermark saturated in a ten per cent solution of salt, and found how many of these must be laid on the tongue in order to produce a taste. A somewhat similar

[26] Virchow's *Arch. f. pathol. Anat. u. Physiol.* vol. 123 (1891), p. 396.
[27] *Ztsch. f. Psychol.*, vol. 26 (1901), p. 383.
[28] *Ergebn. d. Physiol.*, vol. 2, pt. 2 (1903), p. 716.

device was tried by Haemelinck.[29] He used punch discs of blotting paper, but soon abandoned them for a brush. With a single drop of liquid on a dry brush applied to the extended tongue he found that a strength of from 0.2 per cent to 0.3 per cent is needed to produce a sensation.

A brush pencil narrow enough to reach single papillae was employed by Oehrwahl.[30] A brush, a sponge, and a dropper were all used by Schreiber, according to a review by V. Henri in *Année psychol.*, vol. 3 (1897), p. 445. As a result of some 6300 experiments he is reported as giving the following values for the threshold with the solution at 30° C: Tip of tongue, 0.1 per cent; borders, the same; caliciform papillae, 0.08 per cent; whole mouth, 0.5 per cent.

Keppler[31] also used the brush for part of his work, which had to do chiefly with the threshold for difference, but he preferred to take a dose of thirty-five cubic centimeters from a glass. The brush was used by Kiesow[32] for purposes of exploration. He reports average thresholds for six observers as follows: Tip and borders of tongue, 0.25 per cent; base of tongue, 0.28 per cent; but some of the individual threshold determinations run as low as 0.18 per cent. He also used a dose of half a cubic centimeter of liquid.

Hänig,[33] whose work establishes the doctrine of isochymes or zones of equal sensitivity, used first a single drop from a dropper, but later adopted the brush. He found, for five subjects in whom he examined fourteen areas of the tongue, that the threshold ranged from 0.3 per cent to 0.48 per cent, but that most of them were found between 0.35 per cent and 0.4 per cent.

A single drop on the protruded tongue is recommended by Toulouse and Vaschide.[34] Their lowest threshold is 0.9 per cent.

[29] *Année psychol.*, vol. 11 (1905), p. 116.
[30] *Skandinav. Arch. f. Physiol.*, vol. 2 (1891), p. 1.
[31] Pfluger's *Arch. f. d. ges. Physiol.*, vol. 2 (1869), p. 453.
[32] *Philos. Studien*, vol. 10 (1894), p. 362.
[33] *Philos. Studien*, vol. 17 (1901), p. 607.
[34] *C.-R. Acad. Sci.*, Paris, vol. 130 (1900), pp. 803 and 1216.

Vaschide, using the same method, reports the following thresholds for 28 men and 30 women in his article "Gout" in Richet's *Dictionaire de Physiologie*, vol. 7 (1907), p. 666:

For sensation,	men, 0.2%;	women, 1.0%
For discrimination,	men, 1.0%;	women, 4.0%

Fontana[35] dropped three drops on the outstretched tongue and found that a solution of 0.2 per cent was the weakest salt solution which could be perceived clearly and without doubt.

At one time Camerer[36] applied the solutions to single papillae by one or by two capillary tubes. He found that a single papilla stimulated in this way by one part of saturated salt solution in 410 parts of water would give a positive response in half of the cases. This method was also employed by Oerwahl,[37] and it suggests the method extensively used by Sternberg[38] for local exploration, of projecting a fine jet of chloroform or ether vapor against the papilla. Camerer also employed tubes about seven millimeters wide, but it was with this device that he encountered the apparent shift in threshold, due to the effect of the strength of the other stimuli in the series, which effectually prevented his discovering any definite threshold. By dropping one drop containing 0.03 cubic centimeter of solution upon the tongue, he discovered that so little as 0.0024 milligram of salt could be tasted; that is, a solution of 0.8 per cent.

A large number of experimenters have used liquid solutions in larger or smaller quantity. Valentin is quoted as having advised regulating the quantity rather than the concentration of the solution and as reporting that one part of salt in 213 parts of water is perceptible in a dose of 1.5 cubic centimeters, or one part in 426 perceptible in 12 cubic centimeters.[39]

35 *Ztsch. f. Psychol.*, vol. 28 (1902), p. 255.

36 *Ztsch. f. Biol.*, vol. 6 (1870), p. 440.

37 *Skandinav. Arch. f. Physiol.*, vol. 2 (1891), p. 43.

38 *Geschmack und Geruch*, Berlin, 1906; also numerous contributions in periodicals.

39 This is according to the quotation by von Vintschgau in Hermann's *Handbuch*, vol. 3, pt. 2, p. 210, and by Nagel in his *Handbuch*, 1905, vol. 3, p.

Venables[40] found that the most sensitive of two observers could barely detect the presence of salt in one cubic centimeter of a solution of 0.1 per cent. The same amount of solution was used by Kastle[41] in his work on acids. Lombroso and Ottolenghi[42] used only half a cubic centimeter in their tests of criminals and others. Höber and Kiesow[43] found that a molecular concentration of 0.026 was perceptibly salt, not only in the case of NaCl but also for all the other univalent salts tested. Their dose was two cubic centimeters. Kahlenberg[44] used twice as large a dose. Richards[45] gave a small mouthful, perhaps a little more than Kahlenberg. In general the chemists do not seem to share in Valentin's opinion that the quantity of the solution affects the threshold fundamentally.

Camerer at one time used a dose as large as thirty cubic centimeters.[46] An even larger dose was used by Keppler, whose work was published in the same volume as was that of Camerer. In the thirty cubic centimeters Camerer could detect 4.8 milligrams of salt, or 1.6 per cent. In his most elaborate work,[47] Camerer administered a dose of ten cubic centimeters. With this dose the weakest concentration, 0.016 per cent, yielded in each of several extended experiments more positive judgments than did water. A typical set of these observations has already been presented in the text of the present article.[48] The very abundance of the data in this extraordinary thorough investigation of Camerer's precludes the adoption of any particular figure for a threshold. An elaborate explanation and justification would be required

635. The quotation by Vaschide in Richet's *Dictionnaire*, vol. 7, p. 664, seems to be erroneous. The original work of Valentin is not accessible to me; a later edition (1855) gives the threshold for salt at 0.75 per cent without specifying the conditions or the amount of the dose.

[40] *Chem. News*, vol. 56 (1887), p. 221.

[41] *Amer. Chem. Jour.*, vol. 20 (1898), p. 466.

[42] *Ztsch. f. Psychol.*, vol. 2 (1891), p. 346.

[43] *Ztsch. f. Physikal. Chem.*, vol. 27 (1898), p. 604.

[44] *Bull. Univ. Wisc.*, vol. 2, no. 25 (1898), p. 5.

[45] *Amer. Chem. Jour.*, vol. 20 (1898), p. 121.

[46] Pflüger's *Arch. f. d. ges Physiol.*, vol. 2 (1869), p. 323.

[47] *Ztsch. f. Biol.*, vol. 21 (1885), p. 571.

[48] See p. 239 and Plate 4.

for the selection of one intensity rather than a weaker or stronger one.

Heymans[49] also used ten cubic centimeters as a dose. He places the normal threshold at 0.25 per cent.

Bailey and Nichols[50] used a liquid solution which was apparently tasted from a common drinking cup or glass; how much was taken is not stated. They state as the result of observations by 82 men and 46 women that a solution of 0.05 per cent is the weakest that can be tasted.

	Judgments						
Strength of solution, %	"0" or "Water"	"Unknown taste"	"Salt"	"Sour"	"Sweet"	"Bitter"	Other (known) taste
2%	0	0	100	0	0	0	0
1	0	0	99	0	0	0	1
0.5	8	0	86	1	0	3	2
0.25	11	0	76	1	4	3	5
0.125	30	6	47	1	7	5	2
0.0625	52	5	16	2	8	13	4
0	51	6	7	2	9	21	4
0	51	5	1	2	9	27	4
0	46	10	1	4	7	28	4

The observations of 100 students, 32 men and 68 women, already mentioned in Appendix I, are presented here in tabular form for what they are worth. The solutions were presented in an order of descending intensity only, and we should be suspicious that any threshold determined from such a series will be too low. It seems clear that the solution of 0.125 per cent tasted salt to a considerable proportion of the observers, but there is a good deal of doubt about the solution of half that strength for it tasted bitter almost as often as it tasted salt, and it tasted like water to fully half the observers.

This review does not pretend to be complete. It leads to the conclusion that taste-stimuli should be administered in fluid form and in as nearly a normal manner as possible. If the stimulus is so administered, the least perceptible concentration is found

49 *Ztsch. f. Pschol.*, vol. 21 (1899), p. 341.
50 *Nature*, vol. 37 (1887–8), p. 557.

to contain from two to three parts of salt in a thousand parts of water.

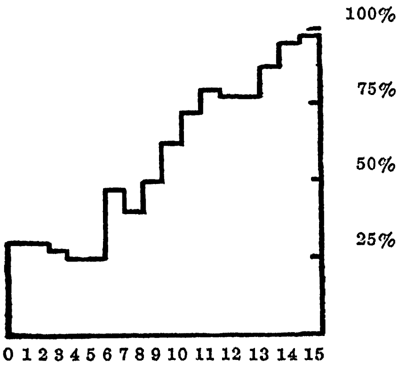
Stimulation by brushes or otherwise, of single papillae, while indispensable in discovering the points and zones of sensitivity for different qualities of taste, is not suitable for quantitative work. In the first place, one never knows how closely the substance is brought into contact with the papilla; if the application is made a little to one side or with a little less force than usual, the substance will not so easily reach the taste organs within. A similar objection holds against the use of pellets or the broad brushing of the tongue, and even against merely dropping the substance upon the protruded tongue. Still more objectionable in quantitative work is the application of the substance in dry form so that it must be dissolved by the more or less abundant fluids already in the mouth.

With regard to the quantity of liquid to be taken into the mouth, it seems to me that there should be enough so that a slight movement of the tongue will bring it into equal contact with all the sensitive parts at once. But the quantity should not be sufficient to disconcert the observer or to give rise to the swallowing reflex.

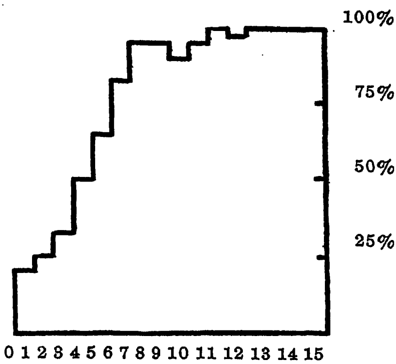
The way of taking the liquid into the mouth is of no great consequence and was determined in the present instance by the greater convenience of pipettes in handling. A small quantity of the fluid in a drinking glass or spoon would first have to be measured out and then the glass rinsed. The pipette combines the two processes in one, and delivers the fluid to the mouth a little more conveniently besides. The slight "taste" due to the odor of the rubber bulb is, on the other hand, a real disadvantage which is not wholly obviated by remarking that all observers quickly learn to disregard it or to treat it as a constant factor.

PLATE 1

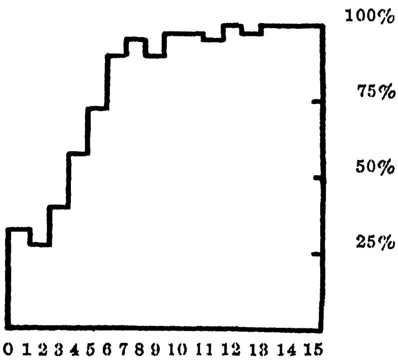
Each figure represents the results of 40 judgments on each of 16 solutions. The strength of solution is laid off along the base line and the unit of strength is one gram of salt per liter of water, or 0.1 per cent. Thus "9" means 0.9 per cent. The number of positive judgments rises to a possible 40 for 100 per cent. The last figure for Obs. Kohlberg represents 80 judgments, including the 40 of the figure next to it.



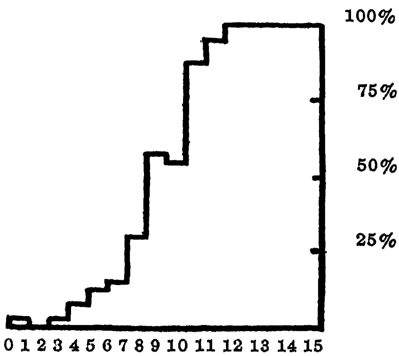
Mr. Bates



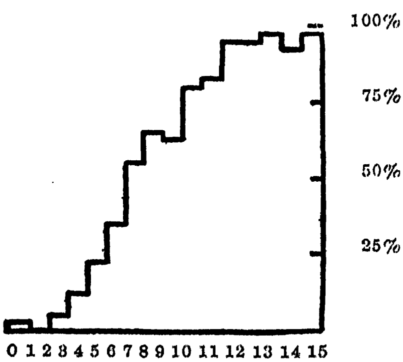
Mr. Detter



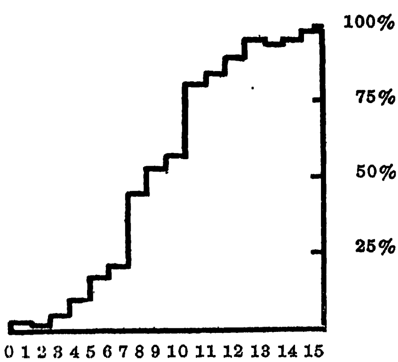
Mr. Ham



Mr. Jackson



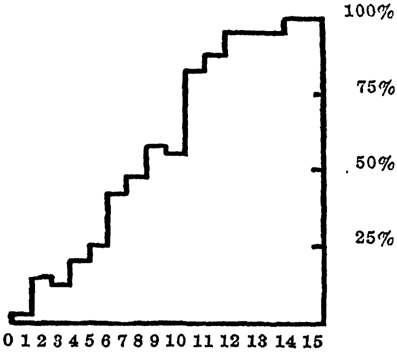
Miss Kohlberg 40



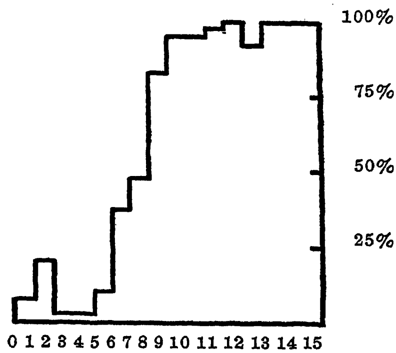
Miss Kohlberg 80

PLATE 2

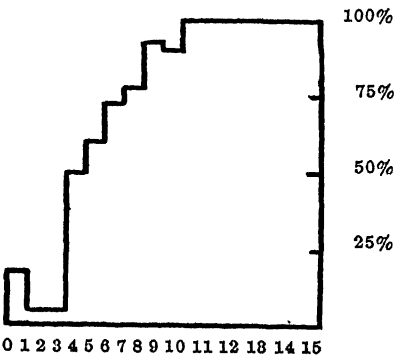
The arrangement is the same as in Plate 1. The last figure is obtained by averaging the number of positive judgments on each solution given by the 10 observers. It rises to a maximum of 398 positive judgments out of a possible 400. See Table XIX for the corresponding numbers of judgments.



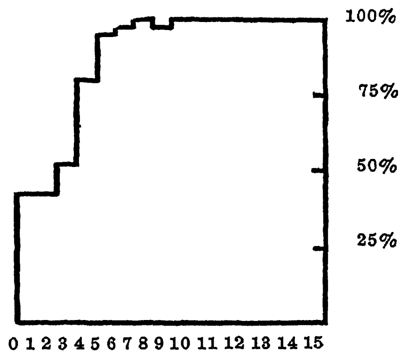
Mr. Libbin



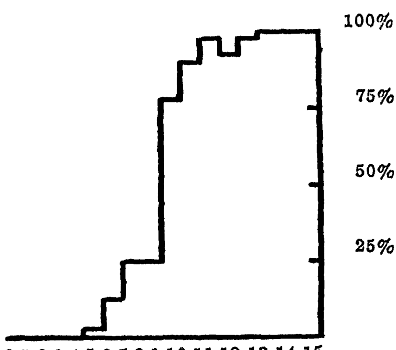
Miss Noteware



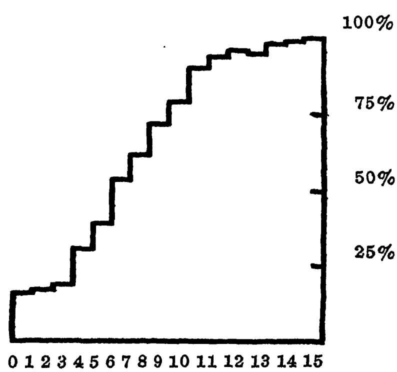
Mr. Robinson



Miss von der Nienburg



Mr. Whisman



Average, 10 observers

PLATE 3

Figures 1, 2, and 3 are constructed on the same principle as those of Plates 1 and 2. In this case the unit of intensity along the base line is one-quarter of a gram of salt per liter of water, or 0.025 per cent. Thus ''9'' represents 9 times 0.025 per cent, or 0.0225 per cent.

Figure 4 corresponds to Table XVIII. For each person the step from some one solution (M) to the next (N) produces a maximum increase in the number of positive judgments. For all the persons the average of this maximum increase (without regard to the strength of M or N) is 8.2 judgments. The average from N to O is 5.8, and from L to M 4.1. This figure bears no direct relation to the ''average'' figure in Plate 2.

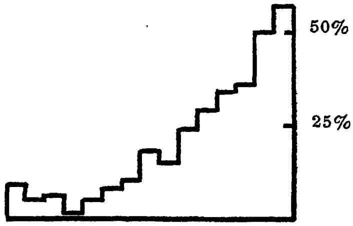


Fig. 1

Miss von der Nienburg

100 judgments on each weak solution

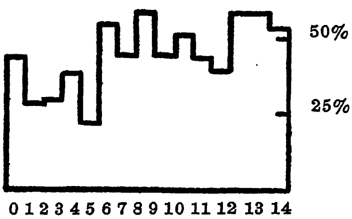


Fig. 2

Mr. Levy

80 judgments on each weak solution

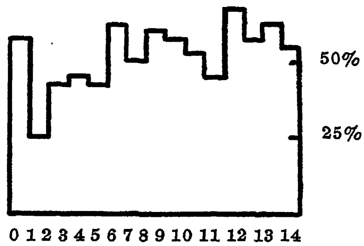


Fig. 3

Miss Noteware

40 judgments on each weak solution

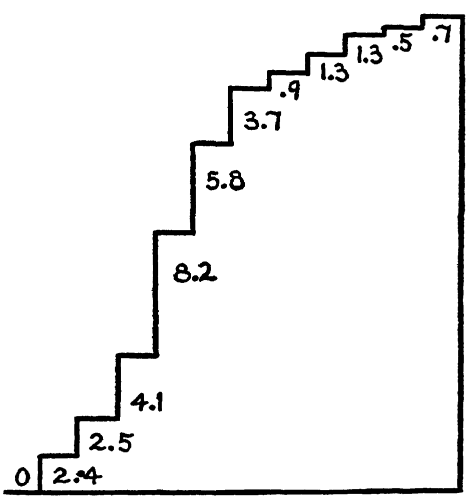
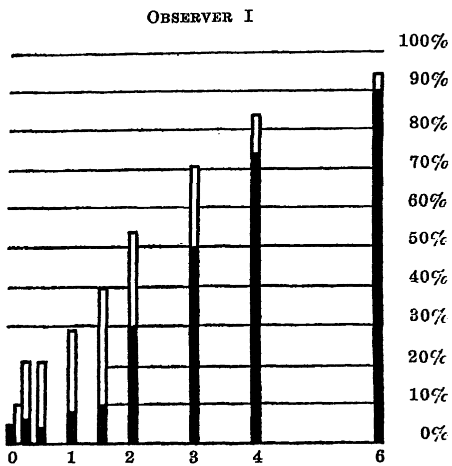


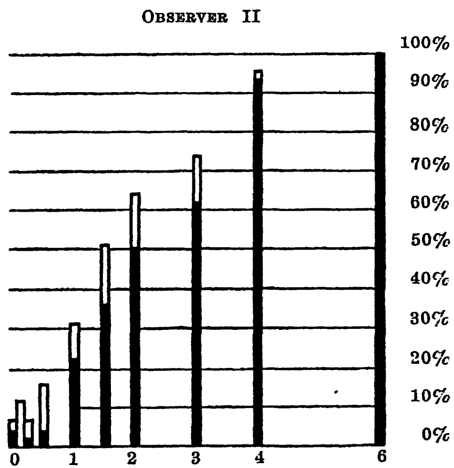
Fig. 4

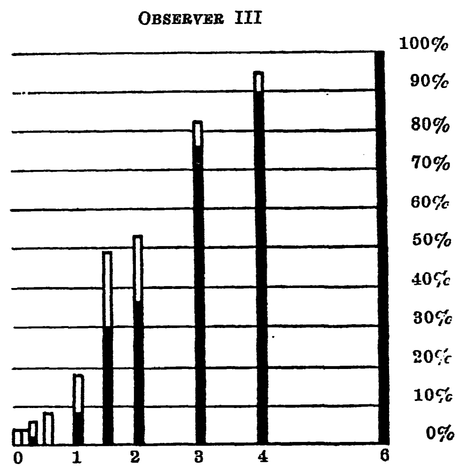
Average Height of the Threshold or Critical Step and of the other Steps

PLATE 4

The three figures represent the work of three of the four observers in one of Camerer's experiments (Ztsch. f. Biol., vol. 21 (1885), p. 578). The intensity of the salt solution is measured off to scale along the horizontal base line. The unit solution is 0.0159 per cent, so that "6" means 6 times 0.0159 per cent, or 0.0954 per cent. The first stimulus is water (0), then comes a solution of one-eighth of the unit, then one of one-quarter of the unit, then one of one-half the unit; then of one, one-and-a-half, two, three, four, and six times the unit strength. The height of the solid line represents the percentage of positive judgments out of a possible 50 for each solution. The height of the outlined portion represents one-half of the number of doubtful cases, also reduced to per cent.


OBSERVER I
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
1
2
3
4
6


OBSERVER II
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
1
2
3
4
6


OBSERVER III
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
1
2
3
4
6

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 4, pp. 269-321 April 24, 1914

HABIT INTERFERENCE IN SORTING CARDS

BY

WARNER BROWN

CONTENTS

I

INTRODUCTION

At various times extending over a period of ten or more years, attempts have been made in the Psychological Laboratory of the University of California to obtain light on the subject of the mutual interference and re-enforcement of antagonistic habits. Numerous card-sorting experiments have been instituted to this end, only to be abandoned upon the discovery that they were leading nowhere. At the beginning cards bearing geometrical designs were used. These were followed by cards which were sorted according to color. These again gave way to ordinary playing cards which were sorted into red and black. Finally the four suits of a pack of playing cards were made the basis of sorting. After this stage had been reached, several unprofitable attempts were made before a satisfactory program was devised with regard to the amount of practice at a sitting, the frequency of practice sittings, and the methods of interfering with established habits. It fell to the lot of the writer to arrange these last details of experimental procedure and to direct and participate in the actual work of experimentation. Credit for instituting the experiments and for outlining their general scope and purpose all belongs to the Director of the Laboratory, Professor Stratton; and to his guiding advice and encouragement is due this publication of the results. To some fifty student volunteers belongs the glory, such as it is, of patient enthusiasm expended without thought of personal reward upon a work of which the value was not immediately appreciable. The actual work of recording the measurements was performed for the most part by five students in the Laboratory, Miss Louise R. Everett, Miss Ruth Pitman, Mr. R. A. Lee, Mr. W. S. Heller, and Mr. Arthur I. Gates. The writer can claim as his own little more than the analysis of the data which have been brought to his

hand by the combined forces of the laboratory. The Pearson coefficients of correlation have been calculated by Mrs. Margaret Hart Strong.

II

PROGRAM OF THE EXPERIMENTS

The experiments fall into two large divisions, one performed in the spring of 1912, the other in the spring of 1913. For the present we need concern ourselves only with the first of these divisions. It involved twenty-six workers, each of whom practiced on thirteen separate days according to the following scheme.

First day's work—One "boxing," which consisted of practice in handling the cards, in the prescribed way, putting them all in one box without sorting. No other preliminary practice was given. Then followed eight successive sortings in the order, diamonds, clubs, hearts, spades. Hereafter this order will be spoken of as the old, or "original," order. It was followed exclusively for eight days of practice. At the end of the first day's work, and at the end of each day's work, a "boxing" record (using the prescribed motion in putting all of the cards into one box, but without stopping to look at the cards) was made. It was thought that this "boxing" record might afford some check on the purely motor factors in the sorting, but the results were disappointing. These records were continued only because they had been included in the work of those persons who did the experiment first.

Second day's work—Eight sortings in the "original" order, followed as always by a "boxing." The boxing comes at the end of every day's work and need not be mentioned again.

Third day's work—Four sortings in the "original" order. On the third, fifth, and seventh days only four sortings were made because of a secondary interest in the effect upon the practice curve of the number of repetitions at a sitting.

Fourth day's work—Eight sortings in the "original" order.

Fifth day's work—Four sortings in the "original" order.

Sixth day's work—Eight sortings in the "original" order.

Seventh day's work—Four sortings in the "original" order.

Eighth day's work—Eight sortings in the "original" order.

Ninth day's work—On this day the factor of interference was introduced. Two sortings were made in the "original" order. Then followed four sortings in the order, clubs, spades, hearts, diamonds. Finally two more sortings were made in the "original" order.

Tenth, eleventh, and twelfth day's work—On these days the work was the same as on the ninth except that a fresh combination of suits was used for the "new" order. As on the ninth day, the four sortings in a "new" order were preceded by two sortings in the "original" order, and followed by two sortings in the "original" order.

Thirteenth day's work—Eight sortings in the "original" order without interference.

Twelve of the twenty-six workers were women. All, both the men and the women, were University students, mostly undergraduates, who volunteered their services for this experiment.

Half of the workers practiced twice a week; the rest came four or five times a week. This irregularity in the schedule was arranged purposely because of a secondary interest in the effect of the frequency of the lessons upon the learning curve. This matter and that of the number of practice trials at a sitting are discussed separately in the last two sections of this paper.

PROGRAM FOR THE THIRTEEN DAYS OF PRACTICE

FIRST DAY	SECOND DAY	THIRD DAY	FOURTH DAY	FIFTH DAY
Boxing				
1. Sorting	1. Sorting	1. Sorting	1. Sorting	1. Sorting
2. "	2. "	2. "	2. "	2. "
3. "	3. "	3. "	3. "	3. "
4. "	4. "	4. "	4. "	4. "
5. "	5. "	Boxing	5. "	Boxing
6. "	6. "		6. "	
7. "	7. "		7. "	
8. "	8. "		8. "	
Boxing	Boxing		Boxing	

SIXTH DAY	SEVENTH DAY	EIGHTH DAY
1. Sorting	1. Sorting	1. Sorting
2. "	2. "	2. "
3. "	3. "	3. "
4. "	4. "	4. "
5. "	Boxing	5. "
6. "		6. "
7. "		7. "
8. "		8. "
Boxing		Boxing

NINTH DAY	TENTH DAY
1. Sorting, original order, D, C, H, S	1. Sorting, original order, D, C, H, S
2. " " " "	2. " " " "
3. Sorting, 1st new order, C, S, H, D	3. Sorting, 2nd new order, H, D, S, C
4. " " " " "	4. " " " " "
5. " " " " "	5. " " " " "
6. " " " " "	6. " " " " "
7. Sorting, original order, D, C, H, S	7. Sorting, original order, D, C, H, S
8. " " " "	8. " " " "
Boxing	Boxing
ELEVENTH DAY	**TWELFTH DAY**
1. Sorting, original order, D, C, H, S	1. Sorting, original order, D, C, H, S
2. " " " "	2. " " " "
3. Sorting, 3rd new order, S, D, C, H	3. Sorting, 4th new order, H, S, D, C
4. " " " " "	4. " " " " "
5. " " " " "	5. " " " " "
6. " " " " "	6. " " " " "
7. Sorting, original order, D, C, H, S	7. Sorting, original order, D, C, H, S
8. " " " "	8. " " " "
Boxing	Boxing

THIRTEENTH DAY
1. Sorting, original order, D, C, H, S
2. " " " "
3. " " " "
4. " " " "
5. " " " "
6. " " " "
7. " " " "
8. " " " "
Boxing

All the sorting for the first eight days was in the same original order, diamonds, clubs, hearts, spades.

III

DETAILS OF EXPERIMENTAL PROCEDURE

The details of the practice work have only an indirect bearing on the results of the experiment—the conclusions are independent of these details—yet no proper conclusions can be reached without a careful planning of the details and a rigid standardization of procedure. The worker sat in front of a straight row of four boxes all built together. Each box had an opening at the top of its vertical face 17 cm. wide by 15 cm. high. The openings were separated by a 1 cm. board. They were 61 cm. from the floor, that is, they were about as high as the elbow of the seated worker. After falling into their respective compartments the cards from all the boxes were led by a chute into a single pile, so that the pack was shuffled automatically. The order of the cards in the pack was therefore entirely a matter of chance and was never twice the same. A considerable amount of preliminary experiment led to the adoption of a prescribed method of handling the cards, which was so simple that it was not subject to change during the course of the experiment. It had been discovered that any change of technique during practice is likely to become a source of large errors, and great care was taken to prevent such a change. The pack of cards was held in the left (or least dexterous) hand, backs up, so that no card was seen until the previous reaction was complete. A card was pushed off by the left thumb, seized by the right hand with the fingers on the back of the card, turned toward the worker, and thrown into the box, face up, without further turning. The experimenter recorded the time with a stop-watch from the word "Go" to the disappearance of the last card. He also recorded the errors, including among them those cards which fell to the floor without entering the box. In

the end the errors were added into the time-record at the rate of one second for each error.[1]

Successive sortings were separated by the time required to pick up the cards and get them all faced down. This time varied somewhat, ranging from a little under two minutes up. Eight sortings and one "boxing" consumed about thirty minutes, and about one minute can be allowed for each sorting. Smooth or ivory-finish "Congress" playing cards with gilt edges were used. Not more than three persons used the same pack of cards. The same person always used the same pack, and the pack was always new at the beginning of the practice, so that all the workers had cards which were about equally worn at any particular stage of the experiment.

On the top of the row of boxes, "ten-spots" of the four suits were displayed in the order in which the cards were to be sorted. These cards were side by side so that the first and last did not lie over the openings in their respective boxes. The workers were given what time they wanted to look over these "samples" each time before beginning to sort. Some people referred to the "samples" more or less frequently while sorting, others never raised their eyes after beginning to sort. Various individual differences were noted in regard to the contents of consciousness while sorting, but these observations do not throw any light on the essential problems of behavior which are primary in this paper.

Throughout the experiments the workers were urged to do their best at all times. The records of all the workers were kept where the workers could hardly avoid seeing them, and each worker was urged to look over the records, so that the spirit of competition was strongly stimulated.

---

1 It takes approximately fifty seconds to sort a pack of cards, and each card dropped or misplaced was considered practically equivalent to a card withdrawn from the pack. The subjects were kept aware of the method of scoring errors and the ideal of an errorless performance was kept before them. Some persons make a good many errors (as many as an average of three in a sitting), some make errors only rarely. Some drop a good many cards through nervousness or clumsiness, some misplace a good many.

An attempt has been made to present the actual results of these experiments together with a rigid analysis of them, but with a minimum of theoretical discussion. The writer has at hand the replies to an elaborately directed introspection made by all of the workers in the first part of the experiment, covering their thoughts, mental attitudes, motor dispositions, images, and, in short, all the immediately observable contents of their minds while working. These documents contain much that is instructive. The writer also has numerous hypotheses concerning the workings of the mental machinery during the process of learning and in the face of interference. Many readers, no doubt, will miss the accounts of the inner mental states, which seem to them at least as valuable as the mathematical measurements, but on the whole it seems wiser to present at this time only what is absolutely impersonal and definitely measureable.

IV

METHOD OF PRESENTING THE DATA

Any particular record of a single trial is subject to considerable chance disturbances, either as the result of conditions affecting the attention of the worker, or because of particularly fortunate or unfortunate sequences of cards. A very slight circumstance may easily so upset a worker that he is temporarily unable to apply himself at top speed to the work. On the other hand, when the cards come so that one follows another in neighboring boxes, a faster record can be made than when they jump back and forth from end to end. When several cards of the same suit fall together the result is generally, but not invariably, a saving in time. In order to avoid so far as possible the evil effects of such accidental disturbances, no single records have been considered, but always an average of at least two records. When more than two were available the best and the worst have been eliminated until two remained, and the average of these

wo taken. For the purpose of analyzing the data the following)oints have been considered, and are here presented in abstract :ogether with the abbreviations used in referring to them in the rarious tables. All of these points are covered in Table I and ıll of the abbreviations will be found in use there.

1. The *speed* of work for any given day (captions A, B, C, D, E, and F in the tables). This is the median record for that day, expressed by the number of seconds required to sort the fifty-two cards into four suits, plus an allowance of one second for each error of any kind. In giving the *rank* with respect to speed the fastest is first, and this fact should be kept in mind when considering correlations between speed and other measurements, because high speed is expressed by a low figure while high measurements of other kinds are expressed by high figures.

2. The *speed at the beginning of the new orders* of the ninth, tenth, eleventh, and twelfth days (caption G) is the average of the first two records in the new order for all of those days together. The *speed at the end of the new orders* (caption H) is the average of the last two records in the new order for all the four days. It will be recalled that there were four records in a new order on each of these days, and that they were the third, fourth, fifth, and sixth records in the eight which constituted the day's work.

3. The *per cent gain from day to day* (captions I, J, K, and L) is found by subtracting the speed on the later day from the speed on the first day and dividing the difference by the speed on the first day. The gain from the eighth to the thirteenth day (caption M) is the difference between the speeds of the eighth and thirteenth days divided by the speed of the eighth day. In giving the *rank* with respect to improvement the one who shows the greatest gains is first.

4. The *rate of attack* on the first day (caption N) is found by taking the difference between the average of the first pair of records and the average of the second pair of records for the first day and dividing by the average of the first pair. The *rate of attack* in the *new orders* (caption O) is found by taking the

TABLE I

Full data for each person, arranged in the order of speed on the first day. For fuller explanation of abbreviations see pp. 277 ff.

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S
	Speed on day						Speed on New Orders at		Per cent gain from day to day				% gain 8th-13th days	Rate of Attack		Loss, %		Errors per trial	
Name	1	2	4	6	8	13	Begin-ing	End-ing	1-2	1-4	1-6	1-8		First Day	New Orders	Old to New	Old to Old	Days 1-8	Days 9-12
Mr. Ot.	52.0	48.0	47.1	45.1	44.4	45.8	48.8	46.4	7.7	9.4	13.2	14.6	– 3.1	10.7	4.8	10.5	4.5	1.29	1.97
Mr. Fish.	55.7	48.9	44.6	46.7	46.0	42.9	51.2	47.3	12.2	20.0	16.1	17.4	6.7	4.3	7.5	12.4	–0.3	0.56	1.31
Mr. Buck.	57.2	54.6	50.9	49.8	48.8	44.3	53.1	51.4	4.5	11.0	12.9	10.7	13.3	4.0	3.3	11.9	–0.8	0.40	0.41
Miss Pit.	58.2	56.0	50.0	49.9	48.3	48.7	58.0	54.2	3.8	14.1	14.2	17.0	– 0.8	1.4	6.5	16.8	0.7	0.79	0.75
Mr. Har.	59.1	59.4	60.0	52.5	51.6	47.1	55.8	51.2	– 0.5	– 1.5	11.1	12.7	8.7	0.0	7.8	13.4	3.1	1.75	1.31
Mr. Gro.	59.2	50.0	43.8	42.8	39.2	43.2	47.0	43.3	15.5	26.0	27.7	33.8	–10.2	3.5	7.8	14.8	4.7	1.00	0.91
Mr. Fro.	59.6	56.1	49.9	49.3	51.8	48.8	53.7	50.9	5.9	16.3	17.3	13.1	5.8	0.0	5.0	8.7	2.1	1.58	2.06
Miss Scot.	60.0	54.0	54.6	51.0	50.5	44.7	52.5	47.1	10.0	9.0	15.0	15.8	11.5	3.3	8.4	10.0	1.3	0.52	0.72
Mr. Cook	60.5	55.1	53.3	48.5	49.6	46.2	53.8	50.7	8.9	11.9	19.8	18.0	6.9	1.9	5.4	15.0	2.9	1.06	1.31
Mr. Jos.	61.0	51.5	46.4	46.9	43.1	44.3	51.9	48.9	15.6	23.9	23.1	29.4	– 2.8	2.6	6.7	15.2	1.7	1.52	1.47
Mr. Dav.	61.6	59.3	51.9	49.2	47.6	44.9	50.9	48.5	3.7	15.7	20.1	22.7	5.7	6.7	4.7	7.0	–0.1	0.67	0.63
Miss Jack.	62.5	59.4	50.5	52.8	55.1	52.0	51.3	50.8	5.0	19.7	15.5	11.8	5.6	2.3	0.9	1.0	0.2	0.90	1.06
Miss McK.	63.5	58.6	51.3	49.3	50.6	47.5	59.4	53.3	7.7	19.2	22.4	20.4	6.1	12.6	9.8	13.3	–3.1	0.48	0.72
Miss Suth.	63.7	55.5	49.2	47.2	48.3	46.6	55.3	48.3	12.9	22.8	25.9	24.2	3.5	14.8	12.9	15.1	–5.8	1.00	3.00
Miss Todd	65.2	57.2	54.6	54.5	53.6	51.9	59.6	53.7	12.3	16.3	16.4	17.8	3.2	– 2.2	8.9	13.7	–1.9	2.18	2.03
Miss Bald.	66.0	55.5	54.3	49.5	48.5	47.8	58.7	54.4	15.9	17.7	25.0	26.5	1.4	– 2.2	6.4	19.6	5.8	0.64	1.40
Miss Ry.	67.1	59.7	56.3	55.8	55.2	48.3	54.0	52.2	11.0	16.1	16.8	17.7	12.5	– 1.0	3.3	7.9	1.5	1.46	1.13
Mr. Hel.	68.1	64.0	56.1	51.8	52.0	50.7	59.6	57.6	6.0	17.6	24.0	23.7	2.5	1.5	3.2	14.6	8.8	1.98	2.69
Mr. Cam.	69.6	63.5	59.1	55.8	57.5	51.9	59.9	55.7	8.8	15.1	19.8	17.4	9.7	7.2	6.7	8.6	1.2	2.77	2.38
Miss Mor.	72.9	61.4	58.7	59.0	58.6	54.7	62.1	58.0	15.8	19.5	19.1	19.7	6.7	10.8	6.4	10.0	0.0	1.60	2.31
Mr. Horn.	73.7	66.0	54.0	56.4	54.0	53.0	61.7	60.0	10.4	26.7	23.5	26.7	1.8	9.6	2.8	22.6	7.3	2.21	2.88
Miss God.	74.0	66.0	59.8	60.0	60.6	59.6	65.2	64.1	10.8	19.2	18.9	18.1	1.7	–15.1	1.6	8.9	8.2	0.62	0.28
Mr. Bos.	78.4	65.2	62.3	58.7	60.5	51.3	57.1	55.6	16.8	20.6	25.1	22.8	15.2	6.9	2.3	.2.9	0.1	1.29	1.60
Mr. Po.	79.4	63.4	58.0	58.0	56.9	47.5	61.2	54.9	20.2	27.0	27.0	28.4	16.5	13.2	7.1	12.0	–4.3	0.54	0.91
Miss Hod.	83.3	69.5	57.5	52.4	52.1	49.0	57.4	56.7	16.6	31.0	37.1	37.4	6.0	14.9	1.0	7.2	3.1	1.54	1.78
Mr. Bris.	86.0	66.0	60.9	60.6	60.0	56.0	65.4	59.9	23.2	29.2	29.5	30.2	6.7	4.1	8.2	15.6	5.6	1.11	1.06
Average	66.1	58.6	53.7	52.6	51.7	48.8	56.3	52.9	10.8	18.2	20.6	21.1	5.4	4.5	5.7	11.9	1.8	1.21	1.46

average of the rates of attack for the four days when new orders were tried, and for each day it is found by taking the difference between the average of the first pair of records in the new order and the second (last) pair of records in that order and dividing by the average of the first pair.

5. The *loss per cent from old order to new order* (caption P) is averaged for the four days when new orders were introduced, and is found by taking the difference between the average of the third and fourth records (which were the first in the new order) and the average of the first pair of records (which were in the old order) and dividing by the average of the first pair (old order). In giving the *rank* with respect to loss the person who loses most is first.

6. The *loss per cent from old order to old order* with practice on a new order intervening (caption Q) is averaged for the four days on which new orders were inserted, and is found by taking the difference between the average of the last pair of records and the average of the first pair of records (four trials of the new order intervening) and dividing by the average of the first pair. In giving the *rank* with respect to loss the person who loses most is first.

7. The *errors per trial* (captions R and S) are found by taking the total number of errors made on the days in question and dividing by the total number of trials made on those days. The errors for the ninth, tenth, eleventh, and twelfth days are given regardless of whether the order was the old or the new.

For the purpose of analysis the device has been adopted of dividing the twenty-six workers into two groups on the basis of their individual records in any of the functions covered by the above headings. One group contains the thirteen persons who attain the highest rank in that particular regard and the other group contains the remaining thirteen persons. This arrangement may be illustrated in the case of the measure of speed on the first day of work (Tables I and II). There are thirteen persons who may be called "fast" in contrast to the other thirteen persons. These thirteen "fast" persons are segregated and

TABLE Ia (*Second Experiment*)

Full data for each person in the second experiment. The abbreviations are the same as explained on pp. 277 ff. for Table I.

	AA	BB	CC	DD	EE	II	JJ	KK	LL	RR
	Speed on day					Per cent gain from day to day				Errors per trial days
Name	1	2	4	6	8	1-2	1-4	1-6	1-8	1-8
Miss Cow.	53.6	48.3	46.8	45.2	45.5	9.9	12.7	15.7	15.1	1.33
Mr. McC.	54.5	51.7	49.0	48.6	46.8	5.1	10.1	10.8	14.1	.75
Miss Whit.	55.3	52.3	50.8	50.0	48.2	5.4	8.1	9.6	12.8	.44
Miss Ack.	55.7	52.3	46.6	46.7	43.4	6.1	16.3	16.2	22.0	2.44
Mr. Sug.	56.9	52.9	50.8	48.7	47.2	7.0	10.7	14.4	17.0	1.25
Miss At.	57.0	54.5	52.3	50.8	45.7	4.4	8.3	10.9	19.9	.98
Mr. Jen.	57.2	47.7	47.6	45.8	43.0	16.6	16.8	20.0	24.8	1.06
Miss Sha.	59.4	55.4	50.2	50.9	49.7	6.7	15.5	14.3	16.3	1.11
Mr. Bol.	59.9	53.6	48.7	49.9	45.6	10.5	18.7	16.7	23.9	1.40
Mr. Hari.	60.6	52.9	49.8	45.9	46.4	12.7	17.8	24.2	23.4	2.94
Mr. Fry.	60.8	55.0	50.8	49.8	50.0	9.5	16.4	18.1	17.8	2.33
Mr. Ga.	61.2	53.2	49.5	47.3	46.3	13.0	19.1	22.7	24.4	2.27
Miss Cock.	61.2	56.4	51.1	47.5	49.3	7.8	16.5	22.4	19.4	1.41
Miss Edg.	62.6	54.5	49.2	49.6	48.5	12.9	21.4	20.8	22.5	1.02
Mr. Ito	64.6	60.5	54.1	51.9	47.9	6.3	16.3	19.7	25.8	1.27
Miss Bog.	69.0	55.7	54.7	48.8	48.3	19.3	20.7	29.3	30.0	.60
Mr. Hig.	74.0	57.1	56.3	54.4	56.5	22.8	23.9	26.5	23.6	1.96
Miss Dy.	74.8	58.2	52.2	50.6	48.3	22.2	30.2	32.4	35.5	.60
Average	61.0	54.0	50.6	49.0	47.6	11.0	16.6	19.2	21.6	1.40

their records in every other respect (speed on the second day, gain from the first to the eighth day, etc.) are averaged (Table II). We are thus in a position to say what may be expected in any given respect of any group of persons who develop better than average speed at the start of their practice work. In this way the twenty-six persons are divided into two rival groups with respect to various aspects of their work.

In addition to this crude method of analysis the Pearson co-efficient of correlation[2] ($r = \frac{\Sigma(x.y)}{\sqrt{\Sigma x^2} \sqrt{\Sigma y^2}}$) has been employed in numerous instances to show the interplay of various measures of attainment (for example, initial speed with amount gained from practice).

The original measurements are too voluminous for publication. All the figures which are to come under discussion are derived from the condensed and analyzed array of Table I. This table shows the data arranged on the basis of speed on the first day (caption A). Similar tables had to be constructed for each of the various functions measured, but there appears to be no need of inserting them.

[2] Thorndike, E. L., *Mental and Social Measurements* (Ed. 2; New York, 1913), p. 178.

V

THE RELATION BETWEEN SPEED AND THE CAPACITY TO LEARN

Dividing the workers into two groups on the basis of their initial ability as measured by the median performance on the first day of practice, we get what may be called a "fast group" and a "slow group" (Tables I and II). The average time required by these two groups to sort the cards is 59 and 73 seconds

TABLE II

Data considered with reference to A, speed on the first day

	Av. record "fast" group	Av. record "slow" group	Correlation between speed and
A. Speed* on first day	59.2	72.9	....
B. Speed on second day	54.6	62.4	.88
C. Speed on fourth day	50.3	57.0	.75
D. Speed on sixth day	48.8	55.2	.79
E. Speed on eighth day	48.2	55.2	.74
F. Speed on thirteenth day	46.2	51.4	.66
G. Speed when beginning new order	52.8	59.7	.74
H. Speed when ending new order	49.5	56.2	.77
I. % gain from 1st to 2nd day	7.7	13.9	.64
J. % gain from 1st to 4th day	15.0	21.4	.65
K. % gain from 1st to 6th day	17.6	23.7	.71
L. % gain from 1st to 8th day	18.2	23.9	.57
M. % gain from 8th to 13th day	4.1	6.7	.36
N. Rate of attack, 1st day	4.1	4.8	.17
O. Rate of attack, new order	6.0	5.4	—.24
P. % loss in passing from old to new order	11.5	12.2	—.09
Q. % loss in old order, due to intervening new order	1.3	2.3	.15
R. Average no. errors per trial, days 1–8	.96	1.46	.21
S. Av. no. errors per trial, days 9–12	1.13	1.80	.14

* Note that high speed is expressed by a small number while large gains or losses are expressed by a large number, so that correlations are really with *slowness* rather than *speed.*

respectively, so that they are clearly contrasted. These two groups maintain their identity remarkably throughout the course of the experiment. On the second day ten of the original thirteen still hold positions in the "fast group." On the fourth, sixth, and eighth days eleven of them are still among the leading thirteen. An individual who is fast at the beginning is apt to remain fast, in comparison with his competitors, throughout the course of practice. There is a high correlation, as indicated in Table II, between speed on the first day and speed on any succeeding day.

But while those who start slow always remain relatively slow there is a distinct tendency on the part of the "slow" individuals to catch up with their faster rivals. This advantage of the "slow" persons with regard to improvement can be seen from the following survey, showing the absolute gain in seconds as well as the relative gain in per cent for each group.

	FAST GROUP		SLOW GROUP	
	absolute	relative	absolute	relative
1st to 2nd day	4 sec. or	8%	11 sec. or	14%
1st to 4th day	9 sec. or	15%	16 sec. or	21%
1st to 8th day	11 sec. or	18%	18 sec. or	24%

Yet even this advantage is transitory. The tendency to catch up is confined to the very first stages of practice. This appears distinctly from the following figures: On the first day the average of the "fast" records is 81 per cent of the average of the "slow" records. On the second day it is 89 per cent of it, showing that the slower persons are making records more nearly equal to those of the "fast" group. But on the fourth, sixth, and eighth days the ratio becomes no more nearly equal, remaining at 88, 89, and 87 per cent on those days.

If we consider the individual persons composing the "fast" group (on the basis of speed on the first day), we find that few of them are included among the quick "learners," or those who improve most by practice. This might also be discovered from the correlation column of Table II, for there it is seen that *low*

speed is distinctly correlated with rapid and large gains from practice.

There are a certain number of persons, however, who succeed in obtaining positions among the rapid learners in spite of the fact that they were among the "fast" workers at the outset.

From the 1st to 2nd day	3	of the original 13 fast workers are among the best learners.
From the 1st to 4th day	5	
From the 1st to 6th day	5	
From the 1st to 8th day	4	

Two of these exceptional persons are very exceptional indeed, for they pass from relatively good positions on the first day (sixth and tenth best) to the very highest positions at the end of practice on the eighth day. These two individuals (Mr. Gro. and Mr. Jos.) stand second and fourth in the rank of those who had made the greatest relative gains during the eight days of practice. Curiously enough, their nearest rivals for honors in this respect are the two individuals who were slowest of all at the beginning of work, and who, consequently had a wide margin left for possible improvement.

Apart from these exceptional cases it seems on the whole unquestionable that those persons who begin slowly will generally manifest a greater capacity for improvement (relative as well as absolute) than those persons who begin to work at a high rate of speed. The effect of practice in this complex process is to flatten out the differences between individuals. Yet evidently we may expect to find a certain number of exceptionally gifted individuals who combine a high capacity for improvement with a better than normal initial performance. Moreover, the tendency of the slow persons to overtake their rivals disappears at an early stage of practice.

Those individuals who work fast at the beginning are less likely to make errors than their slower rivals. Of course, the method of scoring makes it appear that a worker who makes a great many mistakes is slower than another worker who really

makes the same speed but with fewer errors. But the actual number of errors is too small to account for any appreciable part of the slow time of the slower workers. The point is clear that fast workers are much less prone to make mistakes than persons who for any reason work more slowly.

If we start from the point of view of achievement, instead of starting from the initial performance, we obtain new information. Table III shows that while those who gain most at the

TABLE III

Data considered with reference to I, the per cent gain made between the first and second days of practice

	Av. record group of "quick" learners	Av. record group of "slow" learners	Correlation between fast learning and
A. Speed* on first day	70.1	61.9	.64
B. Speed on second day	59.2	58.0	....
C. Speed on fourth day	54.3	52.9	....
D. Speed on sixth day	53.2	50.8	....
E. Speed on eighth day	52.5	50.9	.24
F. Speed on thirteenth day	49.4	48.1	....
G. Speed when beginning new order	57.4	55.3	....
H. Speed when ending new order	53.6	52.1	....
I. % gain from 1st to 2nd day	15.3	6.3	....
J. % gain from 1st to 4th day	22.2	14.2	.76
K. % gain from 1st to 6th day	23.6	17.6	.74
L. % gain from 1st to 8th day	24.8	17.3	.72
M. % gain from 8th to 13th day	5.2	5.7	.03
N. Rate of attack, 1st day	4.2	4.7	.23
O. Rate of attack, new orders	6.2	5.3	.15
P. % loss in passing from old to new order	12.0	12.0	....
Q. % loss in old order, due to intervening new orders	1.4	2.2	....

* Note that *speed* really means *slowness* in the correlations.

beginning of practice are apt to be among the slow starters, this combination does not continue. After the second day the group of "quick learners" make nearly as good time-records as the slower learners who were considerably faster on the first day.

Table IV shows that the same thing is true with regard to the whole gain made during the eight days of practice. Those who gained most were originally rather slow, but after the second day they were hardly any slower than the others. There is no

TABLE IV

Data considered with reference to L, the per cent gain made during the eight days of uninterrupted practice

	Av. record group of "Learners"	Av. record group of "non-learners"	Correlation between capacity to learn and
A. Speed* on first day	70.5 sec.	61.5 sec.	.57
B. Speed on second day	60.1 sec.	56.7 sec.	....
C. Speed on fourth day	54.2 sec.	53.1 sec.	....
D. Speed on sixth day	52.4 sec.	51.6 sec.	....
E. Speed on eighth day	51.6 sec.	51.7 sec.	—.10
F. Speed on thirteenth day	48.9 sec.	48.6 sec.	....
G. Speed when beginning new order	57.4 sec.	55.1 sec.	....
H. Speed when ending new orders	53.7 sec.	51.9 sec.	....
I. % Gain from 1st to 2nd day	13.9%	7.7%	.72
J % Gain from 1st to 4th day	22.8%	13.6%	.80
K. % Gain from 1st to 6th day	25.3%	15.9%	.93
L. % Gain from 1st to 8th day	26.6%	15.5%	....
M. % Gain from 8th to 13th day	4.5%	6.3%	—.29
N. Rate of attack, first day	7.6	1.3	.35
O. Rate of attack, new orders	6.1	5.4	.06
P. % Loss in passing from old to new orders	13.1%	10.7%	.30
Q. % Loss in old order, due to intervening new orders	1.8%	1.7%	.18
R. Av. no. errors per trial, days 1–8	1.20	1.22	.06
S. Av. no. errors per trial, days 9–12	1.64	1.29	.20

*Note that *speed* means *slowness* in correlations.

connection between capacity for benefiting from practice ("plasticity"), and the final speed attained. Nor is there any indication that further practice would change the situation which remained virtually unchanged from the second to the

eighth day of practice. Large practice gains do not insure the attainment of superior skill; on the contrary, they serve as a rule only to reduce the handicap of those persons who are slow at the beginning.

The correlations between the gains made at various stages of practice prove illuminating. Table IV shows that the total gain for eight days is less and less closely connected with gains at earlier stages. This means that the gains at earlier stages were made by persons who failed to hold their own with regard to further gains. The same thing is seen, but less clearly, in Table III. It reflects the fact already indicated—that the tendency for the slow beginners to overtake their more gifted rivals is short-lived.

There is a very slight connection between large gains from practice and a sudden attack (quick improvement) as indicated under Caption N in Tables III and IV.

There is no apparent connection between the amount of practice gain and the number of errors made during the period of regular practice.

Table V gives the reverse view of what we have just seen in Tables III and IV. It is clear that the final speed attained is not closely connected with previous practice gains. Those who have attained the highest speed have not done so by dint of practice. It should be observed, however, that if one may trust the indications of the rather weak correlations in this table, it would appear that further practice would result in a correlation between practice gain and final speed. As the matter stands, it is clear that the speed attained on the eighth day is almost wholly dependent upon the speed at which the person starts, and not on his capacity for improvement by practice.[3]

[3] Lest it be thought that the eight days of practice are not sufficient to enable the slow learners to reach their maximum speed, it should be stated that several of the slower persons have been practiced for longer periods on this work and that none of them has ever made any considerable gain beyond the stage of the eighth or tenth day. The narrow margin remaining for further gains after the eighth day is not sufficient to enable many persons to establish a correlation between final speed and total amount gained.

TABLE V

Data considered with respect to E, speed on the eighth day, the last day of uninterrupted practice

	Av. record "fast" group	Av. record "slow" group	Correlation between speed* and,
A. Speed* on first day	59.8 sec.	72.2 sec.	.74
B. Speed on second day	54.2 sec.	62.8 sec.	....
C. Speed on fourth day	50.5 sec.	56.8 sec.	....
D. Speed on sixth day	48.3 sec.	55.8 sec.	....
E. Speed on eighth day	47.4 sec.	56.0 sec.	....
F. Speed on thirteenth day	45.7 sec.	51.8 sec.	.82
G. Speed when beginning new order	53.5 sec.	59.1 sec.	.77
H. Speed when ending new order	49.6 sec.	56.1 sec.	.81
I. % gain from 1st to 2nd day	9.1%	12.5%	.24
J. % gain from 1st to 4th day	15.3%	21.1%	.15
K. % gain from 1st to 6th day	19.0%	22.3%	.10
L. % gain from 1st to 8th day	20.2%	21.9%	—.10
M. % gain from 8th to 13th day	3.6%	7.2%	.55
N. Rate of attack, 1st day	4.9	4.0	—.12
O. Rate of attack, new orders	7.1	4.4	—.30
P. % loss in passing from old to new orders	13.5%	10.3%	—.34
Q. % loss in old order due to intervening new orders	1.1	2.5	.04
R. Av. no. errors per trial, days 1–8	.90	1.52	.23
S. Av. no. errors per trial, days 9–12	1.23	1.70	.08

* Note that *speed* really means *slowness* in correlations.

Those workers who are fastest on the eighth day are less apt to make errors than slower persons. Thus it appears that freedom from error is associated with capacity for high speed, whether at the beginning or at the end, but that it is not connected with especial capacity for learning.

The results of a second experiment (to be considered in more detail later) under different conditions of practice, support the points so far urged in regard to the relation between speed and the capacity to learn. Table II*a* shows that speed at the beginning is closely correlated with speed at later stages of practice,

TABLE II*a* (*Second Experiment*)

Data considered with regard to AA, speed on the first day

	Av. record "fast" group	Av. record "slow" group	Correlation between speed* and,
AA. Speed* on first day	56.7 sec.	65.4 sec.	....
BB. Speed on second day	52.1 sec.	55.9 sec.	.71
CC. Speed on fourth day	49.2 sec.	52.1 sec.	.76
DD. Speed on sixth day	48.5 sec.	49.6 sec.	.57
EE. Speed on eighth day	46.1 sec.	49.0 sec.	.63
II. % gain from 1st to 2nd day	8.0%	14.0%	.80
JJ. % gain from 1st to 4th day	13.0%	20.3%	.86
KK. % gain from 1st to 6th day	14.3%	24.0%	.86
LL. % gain from 1st to 8th day	18.4%	24.8%	.77
RR. Av. no. errors per trial	1.20	1.60	.43

* Note that *speed* really means *slowness* in correlations.

TABLE III*a* (*Second Experiment*)

Data considered in reference to II, the per cent gain between the first and second days of practice

	Av. record group of "fast" learners	Av. record group of "slow" learners	Correlation between rapid learning and,
AA. Speed on first day	63.7 sec.	58.5 sec.	.80
BB. Speed on second day	53.5 sec.	54.5 sec.	....
CC. Speed on fourth day	50.6 sec.	50.6 sec.	....
DD. Speed on sixth day	48.6 sec.	49.5 sec.	....
EE. Speed on 8th day	47.7 sec.	47.7 sec.	.37
II. % gain from 1st to 2nd day	15.5%	6.5%	....
JJ. % gain from 1st to 4th day	20.2%	13.2%	.84
KK. % gain from 1st to 6th day	23.1%	15.1%	.86
LL. % gain from 1st to 8th day	24.8%	18.3%	.73
RR. Av. no. errors per trial	1.46	1.33	....

and that slow work at the start is correlated with rapid practice gains and with a large proportion of errors. Tables III*a* and IV*a* indicate that those who learn rapidly will attain a speed equal to the speed of their competitors in spite of the slow initial speed of the "learners." Table V*a* shows, as does Table V, that the final attainment of high speed is connected with initial speed but that it is not connected with large gains from practice. Of

TABLE IVa (*Second Experiment*)

Data considered in reference to LL, the per cent gain in speed during the eight days of practice

	Av. record group of "learners"	Av. record group of "non-learners"	Correlation between capacity to learn and,
AA. Speed on first day	64.8 sec.	57.2 sec.	.77
BB. Speed on second day	54.8 sec.	53.2 sec.	....
CC. Speed on fourth day	51:4 sec.	49.8 sec.	....
DD. Speed on sixth day	49.4 sec.	48.8 sec.	....
EE. Speed on eighth day	47.9 sec.	47.3 sec.	.01
II. % gain from 1st to 2nd day	15.1%	6.9%	.73
JJ. % gain from 1st to 4th day	20.5%	12.7%	.83
KK. % gain from 1st to 6th day	23.6%	14.7%	.85
LL. % gain from 1st to 8th day	26.0%	17.2%	....
RR. Av. no errors per trial	1.45	1.33	.00

TABLE Va (*Second Experiment*)

Data considered in reference to EE, speed on the eighth day of practice

	Av. record "fast" group	Av. record "slow" group	Correlation between speed* and,
AA. Speed* on first day	57.4 sec.	64.6 sec.	.63
BB. Speed on second day	51.8 sec.	56.1 sec.	....
CC. Speed on fourth day	49.0 sec.	52.2 sec.	....
DD. Speed on sixth day	47.6 sec.	50.4 sec.	....
EE. Speed on eighth day	45.5 sec.	49.6 sec.	....
II. % gain from 1st to 2nd day	9.5%	12.5%	.37
JJ. % gain from 1st to 4th day	14.5%	18.8%	.34
KK. % gain from 1st to 6th day	16.9%	21.4%	.32
LL. % gain from 1st to 8th day	20.5%	22.6%	.01
RR. Av. no. of errors per trial	1.60	1.19	.01

* Note that *speed* really means *slowness* in correlations.

the nine persons who were classified as "fast" the first day, seven were still "fast" on the last day, and all but two of the fast nine were poor learners. The significant thing is that these more gifted persons hold their lead in spite of their less conspicuous gains. That they do not gain faster is probably due to the near approach of the absolute speed limit beyond which no further improvement is mechanically possible.

A superficial study of Table V*a* gives the impression that those who were fast at the beginning have not been able to retain their immunity from error during the later days. This appearance is largely accidental. The two persons who succeeded by practice in crowding their way into the fast group were both exceedingly prone to make errors. Between them they made 271 errors, as against 81 made by the two workers they displaced. This accounts for the large number of errors made by the fast group on the eighth day.

The analysis so far seems to indicate that the attainment of high speed in such work as this depends on native endowment rather than on capacity for benefiting by practice. Persons who fail to make a relatively good showing at the beginning will not be apt to improve their relative positions, in spite of the fact that they improve rapidly. The only result of their more rapid improvement is a reduction in the absolute distance which separates them from their more gifted competitors. They remain behind in spite of all.[4]

It should be borne in mind that the amount of improvement is narrowly limited by the mechanical conditions of the experiment. No improvement whatever is possible beyond a certain "physiological limit;" hence there is little room for improvement on the part of some of those who begin very fast, while the slow persons have very large possibilities before them. Our fastest record is 36 seconds for the fifty-two cards, or seven-tenths of a second for the complex discrimination and all the movements involved in the reaction.

[4] These conclusions are in substantial agreement with those of Thorndike, *Amer. Jour. Psychol.*, vol. 19 (1908), p. 383, and of Wells, *Amer. Jour. Psychol.*, vol. 23 (1912), p. 75. Compare also, Culler, *Archives of Psychol.*, No. 24 (1912), and Donovan and Thorndike, *Amer. Jour. Psychol.*, vol. 24 (1913), p. 426.

VI

ANALYSIS OF INTERFERENCE

1. The part of the experiment dealing directly with the interference of contrary habits now comes under consideration. The first measure of interference is found in the gain in speed made between the eighth and thirteenth days, practice on antagonistic orders having occurred on the intervening days. The data are presented in Table VI. The conclusion is obvious, indeed it might have been anticipated, that those persons who are capable of making the greatest advances during this period (that is, the persons who are least disturbed by the practice on the new orders) are as a rule slower in absolute speed on all occasions than the persons who find themselves incapable of advancing under such circumstances. In other words, slow people are less likely to suffer from the interference phenomenon than are fast people. Furthermore, the correlations of Table VI give some slight indication of a connection between capacity for learning and the interference phenomenon, intimating that those who have made large gains during the period of regular practice cannot improve much in the face of interference. But the evidence on this point is weak.

It should be observed in passing that the absolute amount of improvement for all persons together is considerably greater from the eighth to the thirteenth days with disturbed practice than from the fourth to the eighth days with uninterrupted practice. That is to say, the ultimate effect upon the group of disturbing the practice is to accelerate rather than to retard the learning process. Careful introspection by a number of the better trained among the workers revealed the fact that the introduction of the new orders had compelled attention to various factors which had remained in the background during the regular

TABLE VI

Data considered in reference to M, the per cent gain in speed from the eighth to the thirteenth day, new orders intervening

	Av. record "unretarded" group	Av. record "retarded" group	Correlation between immunity from disturbance and,
A. Speed* on first day	68.7 sec.	63.4 sec.	.36
B. Speed on second day	59.9 sec.	57.2 sec.	....
C. Speed on fourth day	55.9 sec.	51.4 sec.	....
D. Speed on sixth day	53.6 sec.	50.4 sec.	....
E. Speed on eighth day	53.6 sec.	49.7 sec.	.55
F. Speed on thirteenth day	48.6 sec.	48.9 sec.	....
G. Speed when beginning new order	57.1 sec.	55.5 sec.	.25
H. Speed when ending new orders	53.4 sec.	52.4 sec.	.23
I. % gain from 1st to 2nd day	11.9%	9.7%	.03
J. % gain from 1st to 4th day	17.5%	18.9%	—.17
K. % gain from 1st to 6th day	20.8%	20.4%	—.10
L. % gain from 1st to 8th day	20.6%	21.4%	—.29
M. % gain from 8th to 13th day	9.7%	1.1%	....
N. Rate of attack, first day	6.3	2.6	.17
O. Rate of attack, new orders	5.9	5.6	—.12
P. % loss in passing from old to new order	10.8%	13.0%	—.43
Q. % loss in old order due to intervening new orders	0.8%	2.8%	—.42
R. Av. no. errors per trial, days 1–8	1.16	1.26	.09
S. Av. no. errors per trial, days 9–12	1.30	1.63	.15

* Note that *speed* means *slowness* in correlations.

practice, and these factors proved useful thereafter in the practice on the old order. For example, several persons had failed to observe that the order of cards, diamonds, clubs, hearts, spades, was alternately red and black. After working with a combination that called for the two blacks together and the two reds together, these persons made good use of the idea of alternating colors in the original order.

While the group as a whole continued to acquire speed during the period of interruption, a number of individuals failed to gain speed or actually lost speed. The individual differences range from a gain of 16.5 per cent to a loss of over 10 per cent.

Merely for convenience, we may speak of the persons who suffered from this particular form of interference as "retarded" because they failed to gain from practice under these circumstances.

The "retardation" is particularly noticeable in those exceptional cases in which better than usual initial speed is combined with better than usual improvement from practice. The two workers who combined these qualities in a noticeable degree were among those most retarded by the periods of disturbed practice. Mr. Gro., who by the eighth day had obtained first position in regard to absolute speed and second position in regard to amount of gain in speed, having gained 34 per cent, lost 10 per cent between the eighth and thirteenth days (more than was lost by any other person). The same combination of circumstances occurred in the case of Mr. Jos. almost as strikingly.

The number of *errors* rises noticeably during the days of interrupted practice, and on the thirteenth day it is much greater than on any day preceding the disturbance of practice, as is clearly seen from the accompanying array of figures.

Interference as Indicated by Errors. Actual Number of Errors Each Day

Day		
1.	344	Original order of practice. Average: 1.21 error per trial.
2.	260	
3.	111*	
4.	249	
5.	98*	
6.	216	
7.	97*	
8.	250	
9.	304	Practice interrupted by new order. Average: 1.46 error per trial.
10.	315	
11.	302	
12.	295	
13.	310	Original order again. Av.: 1.64 error per trial.

* Only four instead of eight trials on these days.

What gain is made in the score from the eighth to the thirteenth day is made in spite of a considerable handicap from the larger number of errors on the latter day. Table VI indicates that those who gain most during the interval between the eighth and the thirteenth days have made less errors before the eighth day and will make less errors after the eighth day than their competitors. And they make less errors in spite of the fact that these persons are apt to be "slow," and that the slower individuals as a rule make more errors.

2. A second indication of interference is the inability to pass over from one set of reactions to another without the loss of speed. This factor is considered in Table VII and following tables, and persons peculiarly subject to its influence will be referred to as "disturbed." Certain persons are much more capable of making such a shift than others; in this experiment the least disturbed person took up the new order with a loss of only one per cent in speed, while the most disturbed person lost almost 23 per cent. The loss was measured on each of four days and in order to be sure that it did not vary too much from day to day (for the new order was a different one each day and the worker might grow accustomed, also, to the interruption during the four days) the figures are given separately here for each day. The figures are also given for the corresponding stages on the eighth and thirteenth days (immediately preceding and following the disturbed days) when no change of order occurred, in order to show that the loss of speed is really due to the change of order and not to the stage of the day's work.

8th day, no change of order,	1.2%	gain
9th day, change of order,	12.8%	loss
10th day, change of order,	12.5%	loss
11th day, change of order,	12.0%	loss
12th day, change of order,	10.1%	loss
13th day, no change of order,	0.5%	gain

Dividing the twenty-six workers into two groups as before, but now on the basis of whether they are disturbed or not in

taking up a new order, we find that the "disturbed" persons were also largely "retarded" with regard to the amount of improvement from the eighth to the thirteenth days. The correlation between these two forms of interference (captions P and M in the tables) is relatively high (.43). The persons who were disturbed in passing to a new order only gained about one-third as much during the days of disturbance as the persons who were less disturbed. It also appears that disturbance of this type as well as the other is accentuated in persons who, for any reason, work at high speed. It also appears in this case as before that those persons who gain most from practice are the ones who are most subject to disturbance. Table VIII presents the same

TABLE VII

Data considered with reference to P, the loss in speed in passing from the practiced order to the new order

	Av. record "disturbed" group	Av. record "undisturbed" group	Correlation between amount of disturbance and,
A. Speed on first day	64.6 sec.	67.4 sec.	—.09
B. Speed on second day	57.2 sec.	60.0 sec.	....
C. Speed on fourth day	52.1 sec.	55.1 sec.	....
D. Speed on sixth day	50.4 sec.	53.6 sec.	....
E. Speed on eighth day	49.6 sec.	53.7 sec.	—.34
F. Speed on thirteenth day	48.1 sec.	49.4 sec.	....
G. Speed when beginning new order	56.7 sec.	55.9 sec.	.23
H. Speed when ending new order	52.5 sec.	53.1 sec.	.08
I. % gain from 1st to 2nd day	11.1%	10.5%	....
J. % gain from 1st to 4th day	18.7%	17.6%	....
K. % gain from 1st to 6th day	21.4%	19.8%	....
L. % gain from 1st to 8th day	22.8%	19.3%	.30
M. % gain from 8th to 13th day	2.6%	8.2%	—.43
N. Rate of attack, first day	4.0	4.9	.01
O. Rate of attack, new orders	7.2	4.3	.43
P. % loss in passing from old to new order	15.5%	8.2%	....
Q. % loss in old order due to intervening new orders	2.3%	1.3%	.25
R. Av. no. errors per trial, days 1–8	1.25	1.17	.07
S. Av. no. errors per trial, days 9–12	1.60	1.33	.22

TABLE VIII

Data considered with reference to G, speed when beginning a new order after practice in the old order

	Av. record "fast" group	Av. record "slow" group	Correlation between low speed and,
A. Speed on first day	59.9 sec.	71.8 sec.	.74
B. Speed on second day	54.7 sec.	62.5 sec.	....
C Speed on fourth day	50.6 sec.	56.7 sec.	....
D. Speed on sixth day	49.0 sec.	55.0 sec.	....
E. Speed on eighth day	48.5 sec.	55.0 sec.	.77
F. Speed on thirteenth day	46.1 sec.	51.4 sec.	....
G. Speed when beginning new order	52.2 sec.	60.4 sec.	....
H. Speed when ending new order	49.0 sec.	56.7 sec.	.93
I. % gain from 1st to 2nd day	8.6%	12.9%	....
J. % gain from 1st to 4th day	15.4%	21.0%	....
K. % gain from 1st to 6th day	18.0%	23.2%	....
L. % gain from 1st to 8th day	18.6%	23.6%	....
M. % gain from 8th to 13th day	4.9%	5.9%	.25
N. Rate of attack, first day	4.1	4.8	....
O. Rate of attack, new orders	6.0	5.4	.01
P. % loss in passing from old to new orders	11.0%	12.7%	.23
Q. % loss in old order due to intervening new orders	1.2%	2.4%	.19
R. Av. no. errors per trial	1.06	1.36	....

matter in a somewhat different light. High speed on the new order itself, when beginning, is closely connected with high speed at all stages of the work, so that those persons who have attained a high speed at the end of practice are not, on that account, unable to begin at a high speed on the new order. On the other hand, those who begin the new order at a high speed will not have improved by practice so much before as those who are slower in this, as in the original order. Furthermore the correlation between high speed in the new order and immunity from loss in taking up the new order, while positive, is not close (.23). All this means that certain high-speed persons will experience serious disturbance and consequent loss in transferring to the new order, and yet that they will be able to retain their relative positions still in rank of speed in the new order.

TABLE IX

Data considered with reference to H, speed attained on the new order at the end of practice in it. This table is identical with Table VIII, except the column of correlations

	Av. record "fast" group	Av. record "slow" group	Correlation between speed* and.
A. Speed* on first day	59.9 sec.	71.8 sec.	.77
B. Speed on second day	54.7 sec.	62.5 sec.	....
C. Speed on fourth day	50.6 sec.	56.7 sec.	....
D. Speed on sixth day	49.0 sec.	55.0 sec.	....
E. Speed on eighth day	48.5 sec.	55.0 sec.	.81
F. Speed on thirteenth day	46.1 sec.	51.4 sec.	....
G. Speed when beginning new order	52.2 sec.	60.4 sec.	.93
H. Speed when ending new orders	49.0 sec.	56.7 sec.	....
I. % gain from 1st to 2nd day	8.6%	12.9%	....
J. % gain from 1st to 4th day	15.4%	21.0%	....
K. % gain from 1st to 6th day	18.0%	23.2%	....
L. % gain from 1st to 8th day	18.6%	23.6%	....
M. % gain from 8th to 13th day	4.9%	5.9%	.23
N. Rate of attack, first day	4.1	4.8	....
O. Rate of attack, new orders	6.0	5.4	—.37
P. % loss in passing from old to new orders	11.0%	12.7%	.08
Q. % loss in old order due to intervening new orders	1.2%	2.4%	.37
R. Av. no. errors per trial	1.06	1.36	....

* Note that *speed* means *slowness* in the correlations.

It appears from Table VIII that the correlation is extraordinarily high (.93) between speed when beginning and speed when ending the new orders. As a matter of fact, the same thirteen persons lead in both cases, so that Table IX is identical with Table VIII so far as the grouping of persons is concerned. But the final speed on the new orders is more closely correlated with speed in general, and still less closely correlated with the loss in passing over to the new order.

As already indicated (p. 293), the number of *errors* increases on the days when new orders are practiced. The persons whose time records suffer most from the disturbance in passing over

to the new order make many more errors than the less disturbed persons during the days of interruption, although they had made very few more previous to the disturbance. It should be noted that the discrepancy (Table VII) in the number of errors (about one-third of an error per trial) is inconsiderable in comparison with the difference in the final score (about four seconds per trial).

Interference in this particular phase tends to increase the number of errors while at the same time reducing the speed.

The acquisition of speed in a new order may be more or less subject to interference from the fixed associations of the old order. Table X shows the workers divided into two groups on the basis of the rapidity with which they adjust themselves to the new order. This is measured by the gain in speed from the first pair of records in the new order to the second pair (also in the new order). The figures are averaged for four days, and in order to give assurance of reasonable freedom from error on that account the figures for the separate days and for the same stage of work on the preceding and following days are appended.

8th day, old order, 0.4% gain
9th day, new order, 6.7% gain
10th day, new order, 6.5% gain
11th day, new order, 6.2% gain
12th day, new order, 3.4% gain
13th day, old order, 0.8% gain

The amount of improvement shown in this short time by different individuals ranges from one per cent to thirteen per cent. Those who make the largest gains may be designated "quick beginners." As might be expected, there is a positive correlation between gain in the new order and speed at the end of practice on the new order. There is also a positive correlation with speed in general; that is, the "quick beginners" are also "fast" persons in the regular practice. It should be noted that there is some correlation between the rate of attack in these new orders and the rate of attack on the very first day, as indicated

in Table X under caption N. Apparently there is a characteristic method of beginning new work, some individuals starting abruptly, others hesitating and then plunging quickly in. It is obvious that those who, for any reason, fail to do their best on the initial trial will be able to make a false showing of sudden improvement. This condition may account in part for the dis-

TABLE X

Data considered with reference to O, the rate of attack in the new orders (Per cent gain of the second pair of records over the first pair in the new orders).

	Av. record group of "quick beginners"	Av. record group of "slow beginners"	Correlation between quick beginning and
A. Speed* on first day	65.1 sec.	67.0 sec.	—.24
B. Speed on second day	56.8 sec.	60.2 sec.	....
C. Speed on fourth day	52.8 sec,	54.4 sec.	....
D. Speed on sixth day	51.1 sec.	53.0 sec.	....
E. Speed on eighth day	50.3 sec.	53.0 sec.	—.30
F. Speed on thirteenth day	47.7 sec.	49.9 sec.	....
G. Speed when beginning new orders	56.6 sec.	56.0 sec.	.01
H. Speed when ending new orders	51.6 sec.	54.0 sec.	—.37
I. % gain from 1st to 2nd day	12.1%	9.5%	.15
J. % gain from 1st to 4th day	18.4%	18.1%	....
K. % gain from 1st to 6th day	21.0%	20.2%	....
L. % gain from 1st to 8th day	22.4%	19.8%	.06
M. % gain from 8th to 13th day	4.6%	6.2%	—.12
N. Rate of attack, first day	4.8	4.1	.24
O. Rate of attack, new orders	8.1	3.4	....
P. % loss in passing from old to new orders	13.9%	9.9%	.43
Q. % loss in old order due to intervening new orders	0.7%	2.9%	—.47
R. Av. no. errors per trial	1.14	1.28	....

* Note that *speed* means *slowness* in the correlations.

tinct positive correlation between the rate of attack on the new orders and loss of speed in passing over to the new orders. Persons who are disturbed in beginning the new orders start in upon them slowly, but quickly make great gains, and as they

are apt to be, as we already know, "fast" persons, they soon attain to high speed in the new order. There is the further indication that these persons are more apt to be "learners" than not, and that they are apt to belong to the "retarded" group with regard to their practice gains during the four days of interrupted practice.

It is of passing interest to note that the actual speed when beginning the new orders is faster than on the second day of practice with the old order, and that the rate of attack on the new order is considerably faster than the rate of attack on the first day. In other words, the starting speed and the rate of learning in the new order are both better on account of preceding work in another order.

3. It has been shown that certain persons cannot continue to gain from practice when that practice is interrupted by practice on antagonistic reactions. It is further true that certain persons cannot resume the former reactions at their former speed after being interrupted. It will be remembered that the experiment was so arranged that on four successive days the schedule called for two trials of the old or practiced order, then four trials of a new order, and finally two more trials of the old order. The loss of speed between the first trials (before interruption) and the last trials (after interruption) is set forth in Table XI. The amount of the disturbance ranges in different individuals from a loss of about 9 per cent to a positive gain of nearly 6 per cent. That is, for some persons the interruption helped, or at any rate did not seriously retard, the resumption of work on the old order.

A considerable improvement may be expected between the first and last trials of any day's work without regard to interruptions, and the following figures are included in order that proper allowance may be made for this factor.

8th day,	no	interruption,	1.6%	gain
9th day,		interruption,	1.8%	loss
10th day,		interruption,	1.5%	loss
11th day,		interruption,	1.5%	loss
12th day,		interruption,	2.4%	loss
13th day,	no	interruption,	1.7%	gain

TABLE XI

Data considered with reference to Q, the loss of speed in the practical order on those days when a new order intervened between the beginning and end of practice on the old order.

	Av. record "interrupted" group	Av. record "uninterrupted" group	Correlation between amount of disturbance and
A. Speed* on first day	66.8 sec.	65.1 sec.	.15
B. Speed on second day	59.0 sec.	58.2 sec.	....
C. Speed on fourth day	53.7 sec.	53.5 sec.	....
D. Speed on sixth day	51.6 sec.	52.4 sec.	....
E. Speed on eighth day	50.9 sec.	52.4 sec.	.04
F. Speed on thirteenth day	49.2 sec.	48.3 sec.	....
G. Speed when beginning new order	56.3 sec.	56.3 sec.	.19
H. Speed when ending new orders	53.5 sec.	52.1 sec.	.37
I. % gain from 1st to 2nd day	11.3%	10.3%	....
J. % gain from 1st to 4th day	18.7%	17.7%	....
K. % gain from 1st to 6th day	22.0%	19.2%	....
L. % gain from 1st to 8th day	23.2%	18.9%	.18
M. % gain from 8th to 13th day	2.9%	7.9%	—.42
N. Rate of attack, first day	2.3	6.6	—.48
O. Rate of attack, new orders	4.9	6.6	—.47
P. % loss in passing from old to new orders	13.4%	10.4%	.25
Q. % loss in old order due to intervening new orders	4.6%	—1.0%	....
R. Av. no. errors per trial, days 1–8	1.37	1.05	.26
S. Av. no. errors per trial, days 9–12	1.56	1.37	.07

* Note that *speed* means *slowness* in the correlations.

Merely for the sake of convenience we may speak of those persons as "interrupted" who showed a loss and of those as "uninterrupted" who showed a gain, when the new orders were introduced in this way. Table XI shows that the individuals who were most "interrupted" made small progress from the eighth to the thirteenth day and that their losses were considerable in passing from the old to the new order. They were very slightly slower than the "uninterrupted" persons at the be-

ginning of practice, and during its progress, and more distinctly so at the end of the new orders. They gained more from regular practice than did the "uninterrupted," but they gained less from practice on the new orders. In other words, the "interrupted" persons seem to be typical "learners," but at the same time they are "retarded" as to the advance of the learning process during the period of interruptions and they suffer backsets in beginning upon new work. At the same time their rate of attack upon the new orders is slow.

The "interrupted" persons fall into a few more *errors* than do their less interrupted competitors, but this may be accounted for by the fact that they are slower, and "slow" individuals are prone to make mistakes.

This portrait of the "interrupted" person does not correspond wholly with what we know of the "disturbed" group. The "disturbed" person is fast, and, apparently, his motor co-ordinations are well established. Any disturbance of the conditions of his work upsets the co-ordinations; he is momentarily arrested, then proceeds with alacrity upon the new work. The "disturbed" person experiences difficulty in breaking away from a course of actions to which he is accustomed. The "interrupted" person, on the other hand, is so generally incapable that he performs neither the old nor the new well.

The phenomenon of interference presents both aspects, disturbance with regard to adjustment to new conditions, and interruptions with regard to readjustment to old conditions. The one affects primarily those persons who have attained to at least partial mastery over the necessary co-ordinations; the other affects primarily those persons who are incapable, under the conditions, of developing such mastery.

VII

ANALYSIS OF INTERFERENCE THROUGH INDIVIDUAL TRAITS

Three distinct measures of the interference effect have now been considered:

1. Diminution of practice gains as the result of taking up the practice of new orders.
2. Loss of speed in passing from old to new orders of re-action.
3. Loss of speed in the practiced order due to intervening practice upon different reactions.

These three measures of interference correlate strongly with one another, and yet analysis shows that they rest upon different fundamental traits. Interference is not simple in its operations, but affects different persons differently, according to their native endowment and their capacity for learning.

It may be of some interest to examine in detail certain of the characteristics of the individual persons who are most or least subject to the interference phenomenon in its three chief phases.

In Table XII we see that there is a considerable overlapping in the personnel of the disturbed groups. In any one group there are five persons who are found in both of the other two disturbed groups. In any group there are six more who are found in one of the other groups. And there are two persons in each group who are not found in any other group. There are six persons who are entirely immune from interference. This table shows in this peculiar interlocking of the groups, the basis for the correlations between the various phases of the interference phenomenon.

In Table XIII we have a synopsis of Table XII in so far as it reveals certain of the characteristic traits of the individuals

TABLE XII

Classification of the persons who suffer from different forms of interference.

Person is subject to interference in the form named.	Name.	M Persons most "retarded" from the 8th to the 13th day new orders interspersed.	P Persons most "disturbed" in passing from old to new orders.	Q Persons most "interrupted" in the old order by intervening new orders.	A Speed on 1st day. Fast	 Slow	L Gain from 1st to 8th day. Learners	 Non-learners	O Rate of attack, new orders. Quick	 [illegible]
Subject to M, P, and Q	Mr. Gro.	1	8	6	6		2		6	
	Mr. Jos.	3	5	12	10		4		11	
	Miss Bal.	5	2	4		*16*	7		13	
	Mr. Horn.	7	1	3		*21*	6			[illegible]
	Mr. Hel.	8	9	1		*18*	9			[illegible]
Subject to M and P not Q	Miss Pit.	4	3	*16*	4			*20*	12	
	Miss Tod.	9	10	*23*		*15*		*16*	3	
	Miss Suth.	10	6	*26*		*14*	8		1	
Subject to M and Q not P	Mr. Ot.	2	*16*	7	1			*22*		[illegible]
	Miss God.	6	*19*	2		*22*		*14*		[illegible]
	Mr. Fro.	13	*20*	11	7			*23*		[illegible]
Subject to P and Q not M	Mr. Bris.	*16*	4	5		*26*	3		5	
	Mr. Coo.	*19*	7	10	9			*15*		
	Mr. Har.	*20*	11	9	5			*24*	7	
Subject to M only	Miss Jack.	11	*26*	*17*	12			*25*		[illegible]
	Mr. Dav.	12	*24*	*20*	11		11			
Subject to P only	Miss McK.	*15*	12	*24*	13		12		2	
	Mr. Fish.	*18*	13	*21*	2			*19*	8	
Subject to Q only	Miss Hod.	*14*	*23*	8		*25*	1			
	Miss Ry.	*23*	*22*	13		*17*		*17*		
Not subject to either M, P, or Q	Miss Mor.	*17*	*17*	*19*		*20*		*13*		
	Mr. Cam.	*21*	*21*	*15*		*19*		*18*	10	
	Miss Scott.	*22*	*18*	*14*	8			*21*	4	
	Mr. Bu.	*24*	*15*	*22*	3			*26*		
	Mr. Bos.	*25*	*25*	*18*		*23*	10			
	Mr. Po.	*26*	*14*	*25*		*24*	5		9	

The numbers indicate the position held by the person in order of ranki[ng] with respect to the quality in question. Italics are the positions of pers[ons] not found among the first 13 in rank.

TABLE XIII

Analysis of certain of the Characteristics of the Persons peculiarly subject to, or immune from, the Effects of Interference.

Of the thirteen persons in each group a certain number show the characteristics listed below.	M Persons *most* "retarded" from 8th to 13th day.	P Persons *most* "disturbed" old to new order.	Q Persons *most* "interrupted" return to old order.	M Persons *least* "retarded" from 8th to 13th day.	P Persons *least* "disturbed" old to new order.	Q Persons *least* "interrupted" return to old order.
Fast on the first day	7	7	6	6	6	7
Slow on the first day	6	6	7	7	7	6
Learners 1st to 8th day	7	8	7	5	4	5
Non-learners 1st to 8th day	6	6	6	8	9	8
Both fast and learner	3	3	2	1	1	2
Both fast and non-learner	4	4	4	5	5	5
Both slow and learner	4	5	5	4	3	3
Both slow and non-learner	2	1	2	3	4	3
Quick advance, new order	8	10	5	7	3	8
Slow advance, new order	5	3	8	6	10	5

who are either seriously or only slightly disturbed by the interference phenomenon in the three ways indicated. The points brought out are the same as already discovered by statistical methods, but it may be worth while to reiterate them here, chiefly perhaps for the benefit of those readers who have a temperamental distrust of coefficients of correlation.

1. Of the "retarded" persons who make the least headway over the period during which they practice new reactions, and of the "disturbed" persons who encounter a difficulty in taking up a new set of reactions, there are more individuals who start at a high rate of speed than at a low. But just the opposite is true of the "interrupted" persons who find difficulty in resuming the accustomed order after a change.

2. It is very clear that those who suffer from interference of any kind are much more likely to be "learners," who have gained considerably from practice, than not. And the majority

of those who are immune from interference are non-learners. This is particularly true of the disturbance in passing from a practiced to a new order.

3. The rare combination of fast worker and good learner is peculiarly liable to suffer from interference, while the combination of slow work and inability to learn secures practical immunity from interference, particularly in passing to a new order. Such a stupid person can do anything almost as well as that upon which he may be engaged at the moment.

4. The most of those who are disturbed in approaching a new order, or whose advance is retarded by practice upon it, are able to advance rapidly in the new order itself. But most of those whose day's work is interrupted by the interjection of new orders fail to advance much in the new order.

VIII

THE EXTENT OF THE PHENOMENA OF INTERFERENCE

The part of the experiment which has been so far considered was devised primarily to permit of an analysis of the gifts and capacities underlying the phenomena of interference. Incidentally, however, certain quantitative observations could not be avoided. Thus it is clear that practice beyond the eighth day results in a considerable improvement in speed in spite of the fact that regular practice is disturbed by the intrusion of practice on antagonistic reactions. The actual amount of the improvement from the eighth to the thirteenth day is considerably greater than the amount from the fourth to the eighth.

Speed 3rd day, 56.2 sec.	Speed 4th day, 53.6 sec.	Speed 8th day, 51.7 sec.
Speed 8th day, 51.7 sec.	Speed 8th day, 51.7 sec.	Speed 13th day, 48.8 sec.
Gain...... 4.5 sec.	Gain...... 1.9 sec.	Gain...... 2.9 sec.

Thus it appears that constant interference with practice does not prevent the steady growth of habit. Unfortunately the nature of the interference itself prevents the determination of any points on the practice curve between the eighth and thirteenth days. No adequate record of the speed could be obtained on those days because the speed was affected by the interference and because the usual number of regular records could not be made while the interference records were being made.

A second indication of the amount of the interference effect appears in the fact that the speed obtained in the new orders, which are begun directly after the practice on the accustomed orders, is better than the speed at which work was originally begun. It is nearly equal in fact to the speed obtained on the third day of practice in the original order.

Speed 1st day, original order,	66.0 seconds
Speed 3rd day, original order,	56.2 seconds
Speed when beginning new order,	56.3 seconds
Speed when ending new order,	52.9 seconds
Speed 6th day, original order,	52.6 seconds

Moreover, the improvement shown in the new orders as between the first two trials and the second (last) two trials is as great as that shown in three days of regular practice. Any interference which may appear in individuals is overshadowed in the whole group of workers by transference of skill from one set of reactions to another; four trials of any new order suffice to acquire a speed that it required six days, or forty trials, to acquire in the original practice.

The observation of these facts raised the question whether the learning of a second set of reactions to identical stimuli in any way interferes with the execution of the accustomed reactions. The classical experiments of Münsterberg[5] show that both sets of reactions may exist side by side, but unfortunately he does not consider the loss of time which may possibly be involved in instituting one reaction rather than the other.

[5] Münsterberg, H., *Beiträge zur Experimentellen Psychologie,* (Freiburg i. B., 1892), Heft 4, p. 69.

The relatively large interference found by Bergström[6] must be largely discounted in view of the subsequent and much more radical experiments of Bair.[7] For some reason a large part of the work upon this topic has been devoted to entirely secondary points, such as the dependence of the interference effect upon the interval elapsing between trials, or upon the number of practices given at a stretch before the change, or the total number of practices preceding the change. The recent paper of Culler[8] only partly presents what is really the practical question underlying all this work: Is the advance of learning in one set of reactions seriously (or at all) disturbed by learning a different set of reactions to the same stimulus? In other words, can we learn to do a thing a new way without impairing the growth of the habit of doing it in the accustomed way?

In order to answer this question more clearly a second set of experiments was instituted in the spring of 1913. Eighteen volunteer student workers were drawn from classes corresponding to those of the previous year. Every precaution was taken to have the conditions exactly the same as before.

Practice was given for eight days in sorting the cards in the "old order" according to the schedule arranged the year before. This practice (twice a week) corresponded exactly in every respect with the first eight days of practice in 1912. There was just one difference, and that was that each day of practice was followed, twenty-four hours later, by an equal amount of practice in sorting the cards in the order: clubs, spades, diamonds, hearts. The result is that while the workers of this group had the same practice in the old order as their predecessors they had *also*, on alternate days, an equal amount of practice in another order.

Table XIV presents a comparison between the crude results of the two experiments. Allowing for certain irregularities in both groups on the eighth day, it appears from this table that the workers in the second, or "interference," group were not able

[6] *Am. Jour. Psychol.*, 5 (1893), p. 356 and 6 (1894), p. 433.

[7] *The Practice Curve*, New York, 1902.

[8] *Archives of Psychol.*, No. 24, 1912.

TABLE XIV

COMPUTED FOR THE ENTIRE NUMBER OF PERSONS IN EACH EXPERIMENT

Average Speed for each Day of Practice.

Day	1	2	3	4	5	6	7	8	
Normal practice group	66.0	58.5	56.2	53.6	53.3	52.6	51.2	51.7	
Interference group	61.0	54.0	52.3	50.6	49.7	49.0	49.0	47.6.	
Difference	5.0	4.5	3.9	3.0	3.6	3.6	2.2	4.1	in favor of interference

Average per cent Gain from Day to Day

Day	1–2	2–3	3–4	4–5	5–6	6–7	7–8	1–4	1–6	1–8
Normal practice group	10.8	4.0	4.3	0.6	2.2	1.6	—1.1	18.1	20.6	21.1
Interference group	11.0	3.2	3.1	1.6	1.4	0.1	2.8	16.6	19.1	21.6

Rate of Attack, i.e., Gain between the 1st pair and the 2nd pair of Trials

Day	1	2	3	4	5	6	7	8
Normal practice group	4.5	1.7	3.8	2.4	2.1	1.4	0.1	1.2
Interference group	8.2	5.9	5.2	4.3	4.7	3.8	3.6	3.7

Actual Number of Errors per Day

Day	1	2	3	4	5	6	7	8	Total, 8 days.
Normal practice group	13.2	10.0	4.3	9.6	3.8	8.3	3.7	9.6	63
Interference group	11.9	11.4	4.4	11.9	5.7	11.6	5.1	10.7	73

to maintain their original superiority in speed over the workers who had uninterrupted practice the year before. Interference does seem to manifest itself slightly to the detriment of those who were trying to learn to sort the cards both ways.

But some allowance ought to be made for the fact (Table XIV) that there happened to be more "fast" individuals (in regard to initial speed) in the second experiment than there were in the first. This fact rather than the element of interference might easily be responsible for the somewhat slower improvement of the second experiment, for as a rule fast persons are not as capable of improvement as slow ones. In order to get rid of this objection and make the two groups more directly comparable a fresh computation has been made in which certain persons have been eliminated from each group. From the second group have been taken four of the six fastest persons, leaving two who make better than 57.2 seconds as in the first group. From the workers in the first group all have been eliminated who could not be paired (approximately) with persons in the second group. There remain 14 persons in each experiment. The aggregate time is exactly the same for the two groups and each individual in one group is closely paired with some individual in the other group. The elimination has been made wholly on the basis of the speed on the first day without any regard to the subsequent performance of those who remain or of those who are eliminated.[9]

Table XV shows the method of elimination. Table XVI shows the comparison of the two groups of workers after eliminating to make the two groups strictly comparable. It now appears that the group subjected to interference actually learned faster than the group which had uninterrupted practice. Apparently the practice on the second order helped in the learning of the first order. It is particularly interesting that the

[9] The great difference in the initial capacities of these two groups, the members of which came from precisely the same classes of students on two successive years, cannot fail to impress one with the danger of over hasty generalization from data obtained from a comparatively small number of individuals. Even a low variation is no guarantee that another group of persons will not present an entirely different range of capacities

TABLE XV

Showing the initial speed of all the persons in both experiments, and showing which persons were eliminated because no person of a corresponding speed could be found in the other experiment.

First experiment Name	Speed		Second experiment Name	Speed
Mr. Ott.	52.0	....	Miss Cow.	53.6
			Mr. McC.	54.5
			Miss Whit.	55.3
Mr. Fish.	55.7	....	Miss Ack.	55.7
			Mr. Sug.	56.9
			Miss At.	57.0
Mr. Buck	57.2	....	Mr. Jen.	57.2
Miss Pit.	58.2			
Mr. Har.	59.1			
Mr. Gro.	59.2			
Mr. Fro.	59.6	....	Miss Sha.	59.4
Miss Sco.	60.0	....	Mr. Bol.	59.9
Mr. Cook	60.5	....	Mr. Hari.	60.6
Mr. Jos.	61.0	....	Mr. Fry.	60.8
Mr. Dav.	61.6	....	Mr. Ga.	61.2
Miss Jack.	62.5	....	Miss Cock.	61.2
Miss McK.	63.5	....	Miss Edg.	62.6
Miss Suth.	63.7	....	Mr. Ito	64.6
Miss Todd	65.2			
Miss Bald.	66.0			
Miss Ry.	67.1			
Mr. Hel.	68.1			
Mr. Cam.	69.6	....	Miss Bog.	69.0
Miss Mor.	72.9			
Mr. Horn.	73.7	....	Mr. Hig.	74.0
Miss God.	74.0	....	Miss Dy.	74.8
Mr. Bos.	78.4			
Mr. Po.	79.4			
Miss Hod.	83.3			
Mr. Bris.	86.0			
Av. before elimination	66.0	....		61.0
Av. after elimination	62.4	....		62.4

TABLE XVI

COMPUTED FOR 14 PERSONS OF APPROXIMATELY EQUAL SPEED IN EACH OF THE TWO EXPERIMENTS

Average Speed for each Day of Practice.

Day	1	2	3	4	5	6	7	8	
Normal practice group	62.4	56.8	54.5	51.5	51.8	50.6	49.8	50.7	
Interference group	62.4	54.3	52.2	50.5	49.4	48.8	49.2	47.7	
Difference	0.0	2.5	2.3	1.0	2.4	1.8	0.6	3.0	in favor of interference

Average per cent Gain from Day to Day

Day	1–2	2–3	3–4	4–5	5–6	6–7	7–8	1–4	1–6	1–8
Normal practice group	8.9	3.9	5.3	—0.4	2.1	1.4	—1.8	17.1	18.8	18.6
Interference group	12.6	3.9	3.0	2.3	2.5	—0.6	2.8	18.7	21.4	22.4

Rate of Attack, i.e., Gain between the 1st pair and the 2nd pair of Trials

Day	1	2	3	4	5	6	7	8
Normal practice group	4.6	0.7	3.6	2.7	1.7	1.0	—0.2	1.9
Interference group	9.8	6.0	6.1	5.4	5.7	5.0	4.6	3.3

Actual Number of Errors per Day

Day	1	2	3	4	5	6	7	8	Total, 8 days.
Normal practice group	12.7	7.9	3.4	8.1	3.9	8.3	3.9	9.5	58
Interference group	13.1	12.6	5.0	14.1	6.1	12.6	5.4	11.8	81

second day shows a much larger gain over the first when a new order of work has intervened. This agrees perfectly with the theory that the learning process was facilitated in the first experiment on those days when new orders were practiced.

The inference to be drawn from these experiments is that learning to do a thing in two different ways is not detrimental. It may even be helpful. How well the second order was learned we do not know. The cards could be sorted in the second order more quickly and with less feeling of effort than in the "old order." There is no ground, therefore, for comparative measurements.

While interference is wholly lacking as regards the steady development of skill in sorting the cards, yet it manifests itself in two ways unmistakably. Both Tables XIV and XVI show that the "rate of attack" was much faster with interference than with normal practice. That means that there was a loss of speed at the beginning of each day's work due to the persistence of antagonistic tendencies from the order which had been practiced on the preceding day. The daily records are based on medians from which the extremely long and extremely short records have been excluded. In all probability the average for the day would be influenced by the slow records at the beginning of the day's work, and an array of figures computed from averages might show an interference effect upon the practice gain. It is true, however, as shown here that the interference phenomenon is confined to a few trials on first beginning work, and does not effect the increase of skill in performing the action in the accustomed manner.

Interference shows itself again unmistakably in an increase in the number of errors. It has already been mentioned (p. 293) that the errors increased in the first experiment on those days when two different orders were practiced and also on the thirteenth day when the old order was resumed after four periods of interruption. It is clear from Tables XIV and XVI that the number of errors was considerably greater in the second experiment (interference) than in the first (normal practice). No such

difference was discoverable on the first day of practice in the new experiment and no ground for its appearance on the later days can be found, except the antagonistic tendencies which persisted from practice on the interpolated order. Unfortunately it was not found practical to keep a separate record of those mistakes which Culler aptly calls "relapses."[10]

IX

SEX DIFFERENCES

Table XVII is arranged to show what differences there are in these experiments between men and women. The first experiment seems to give very definite results. The men are faster. They do not learn so fast at first but they learn more toward the end. The men attack the work more quickly at the beginning (first day). They suffer much more loss in the old order due to interruption by the new order. They suffer more loss in passing to the new order. They do not gain so much in the new order, but they gain slightly more during the disturbed days from the eighth to the thirteenth. The men make more errors at all times. From all of this numerous interesting inferences might be drawn. But the second experiment fails to confirm the first in nearly all of these respects. It shows no sex differences of any consequence except that the men still make more mistakes. We are confronted with one of those cases in which the temptation to generalize is nipped in the bud by the failure of the experiment to permit of repetition. Inferences with regard to such matters as sex differences are extremely dangerous at best, and the result shows in this case how even quite clear figures resting on a fairly large number of cases may prove to be misleading.

Something of the same sort happened in the case of a correlation which was established in the first experiment between youth

[10] *Archives of Psychol.*, No. 24 (1912), p. 59.

and capacity for improvement. No such correlation appeared in the second experiment.

TABLE XVII

Data considered with reference to Sex

	First experiment		Second experiment	
	Men	Women	Men	Women
A. Speed on first day	65.4	66.9	61.0	61.0
B. Speed on second day	58.0	59.3	53.7	54.1
C. Speed on fourth day	53.2	54.2	50.7	50.4
D. Speed on sixth day	51.4	52.8	49.2	48.9
E. Speed on eighth day	50.8	52.8	47.7	47.4
F. Speed on thirteenth day	47.8	50.0		
G. Speed when beginning new order	55.4	57.5		
H. Speed when ending new order	52.1	53.8		
I. % gain from 1st to 2nd day	10.6	11.1	11.5	10.5
J. % gain from 1st to 4th day	17.9	18.6	16.6	16.6
K. % gain from 1st to 6th day	20.7	20.6	19.2	19.0
L. % gain from 1st to 8th day	21.4	20.6	21.6	21.4
M. % gain from 8th to 13th day	5.6	5.2		
N. Rate of attack, first day	5.1	3.6		
O. Rate of attack, new orders	5.5	6.0		
P. % loss in passing from old to new orders	12.3	11.2		
Q. % loss in old order due to intervening new orders	2.4	0.9		
R. Av. no. errors per trial, days 1–8	1.32	1.06	1.69	1.10
S. Av. no. errors per trial, days 9–12	1.52	1.38		

THE INFLUENCE OF THE INTERVAL BETWEEN SITTINGS

Numerous experiments have given indications of a law of diminishing returns in the process of learning, through the operation of which each of a series of repetitions is rendered less effective than an earlier repetition in the series. And at the same time, it has been conclusively shown that repetitions are more effective which are well separated from one another in time. Thus the

maxim has been accepted that the economy of learning is promoted by making only a few repetitions at rather infrequent intervals. Most of the experiments upon which these conclusions are based have been concerned with that type of learning which is manifested in rote memory and more particularly the learning of nonsense syllables or of codes. On the other hand, the common practice of those who teach typewriting, telegraphy, and the playing of musical instruments seems to indicate a belief in the effectiveness of numerous and frequent repetitions in that type of learning which involves the automatization of motor responses.

The program for the first of the two card-sorting experiments was so arranged that half the workers practiced five times each week while the other half of them practiced only twice a week (i.e., either Monday and Wednesday or Tuesday and Thursday). Improvement is measured from the first to the eighth day (the last day of undisturbed practice) in per cent of the first day's speed. No very clear results appear. The improvement shown by the slow individuals weighs too heavily in the average, and the range of achievement (some improve three times as much as others) is too great, to permit of a fair comparison of the results obtained from the two schedules.

The results as they stand (Table XVIII) indicate that the persons taking the daily schedule were, on the average, slower than the others, and so had a better chance of improving, and yet their relative gain is less. But this result is so evidently dependent upon the work of certain individuals that no reliance can be placed in it.

If instead of the average gain we consider the median, we find that the improvement by practice twice a week is not so great as by daily practice. If we eliminate from each group all the individuals who cannot be closely paired with an individual in the other group, we have left only five in each group. These five are starred in Table XVIII. Starting with approximately the same initial speed, the five who worked twice a week made greater gains than the five who worked daily.

TABLE XVIII

The advantage of daily practice over practice twice a week. The gain calculated from the median record of the first day to the median record of the eighth day.

Name	Daily practice: Speed 1st day	Daily practice: Gain % from 1st to 8th day	Name	Practice twice a week: Speed 1st day	Practice twice a week: Gain % from 1st to 8th day
Mr. Har.*	59.1	12.7	Mr. Ot.	52.0	14.6
Miss Scott.*	60.0	15.8	Mr. Fish.	55.7	17.4
Mr. Jos.*	61.0	29.4	Mr. Buck	57.2	10.7
Mr. Dav.	61.6	22.7	Mr. Gro.*	59.2	33.8
Miss McK.*	63.5	20.4	Mr. Fro.*	59.6	13.1
Miss Todd.	65.2	17.8	Mr. Coo.	60.5	18.0
Miss Bal.	66.0	26.5	Mr. Doug.*	60.9	14.6
Miss Ry.	67.1	17.7	Miss Jack.	62.5	11.8
Mr. Hel.	68.1	23.7	Miss Suth.*	63.7	24.2
Mr. Cam.	69.6	17.4	Mr. Po.*	79.4	28.4
Miss Mor.	72.9	19.7	Miss Hod.	83.3	37.4
Mr. Horn.	73.7	26.7	Mr. Bris.	86.0	30.2
Miss God.	74.0	18.1			
Mr. Bos.*	78.4	22.8			
Average	70.0 sec.	20.8%		65.0 sec.	21.2%
Median	66.5	20.0		60.7	17.7
Av. starred or paired persons	64.4	20.2		64.5	22.8

NOTE.—Table XVIII omits Miss Pit., who practiced three times a week, and includes Mr. Doug., who does not appear in previous tables because his work did not extend beyond the first eight days.

XI

THE INFLUENCE OF THE NUMBER OF REPETITIONS AT A SITTING

It will be recalled that the schedule (see page 273) of the practice work called for only four trials on the third, fifth and seventh days, whereas there were eight trials on all the other days. It was the object of this arrangement (which may have proved rather confusing to the reader at times) to discover whether the improvement resulting from four rounds of practice was appreciably less than that resulting from eight rounds.

The possible advantages to be derived from increasing the number of repetitions made at a sitting are of two sorts. In the first place, the prolongation of the practice may result in the achievement of a higher level of efficiency at that time. Having reached this level, one expects to be able to advance beyond it on a subsequent occasion. One aim of practice is to increase skill as much as possible at each lesson, and conceivably, this purpose may be promoted by prolonging the process of repetition.

The data for the second, fourth, sixth and eighth days, on each of which eight practice records were made, indicate that there is a slight chance that a better single record will be made in the second half of a forty-minute period of practice than in the first half, but that this chance is less after the stage of most rapid learning has been passed. Table XIX is based upon a comparison of the best of the first four records and the best of the last four. If the continuation of the practice is immediately profitable, a better record will be found among the last four records than has been made in the first four. On the second day of practice there is a very considerable chance of doing better in the second half of a forty-minute period. This is undoubtedly due to the fact that the improvement is still very rapid on the second day. There is still much which can be learned during the hour. But after that day practice becomes more a matter of routine and the improvement during the sitting is comparatively slight.

The results of practice which are not immediately gathered in the form of greater skill at the end of the hour may be looked for on the following day. Is it true that longer practice gives greater facility on the following day? If so, it should be true that the records for the day following a long practice show a greater improvement over the records of the preceding days than is shown after a short day's practice. Here the results are ambiguous and various modes of presentation have been adopted in Table XX in order to present the actual facts with as little distortion as possible.

TABLE XIX

SHOWING WHAT PROPORTION OF THE BEST RECORDS MADE IN A DAY LIE IN THE SECOND HALF OF THE PERIOD OF PRACTICE

Each of the 31 persons made his best record for the day in either the first half or the second half of the sitting. The table shows the number of these best records occurring in each half and compares the sum of the best records of the first half with the sum of the best records of the second half.

Second day's work	25	of the	best	records	in the second half	are better than	the	best	records	in the first half	by	68.6 secs.
	5	"	"	"	"	are worse than	"	"	"	"	by	6.0 secs.
	1	"	"	"	"	is equal to	"	"	"	"		
Fourth day	19	"	"	"	"	are better than	"	"	"	"	by	35.8 secs.
	12	"	"	"	"	are worse than	"	"	"	"		22.4
Sixth Day	20	"	"	"	"	are better than	"	"	"	"		35.8
	8	"	"	"	"	are worse than	"	"	"	"		19.0
	3	"	"	"	"	are equal to	"	"	"	"		
Eighth day	17	"	"	"	"	are better than	"	"	"	"		40.8
	13	"	"	"	"	are worse than	"	"	"	"		30.0
	1	"	"	"	"	is equal to	"	"	"	"		

TABLE XX

Comparison of the returns from a short day's work (4 trials) with the returns from a long day's work (8 trials).

Gain from day to day	Gain from the median of one day to the median of the next day.	Gain from the median of the first 4 trials each day to the median of the first 4 trials the following day.	Gain from the median of the first 4 trials each day to the best record of the first 4 trials on the following day.	Gain from the median of one day to the median of the first 4 trials on the following day.
Second day (long) to third day (short)	3.6%	5.9%	9.8%	3.6%
Third day (short) to fourth day (long)	4.4	4.3	7.9	4.3
Fourth day (long) to fifth day (short)	0.4	0.6	4.5	0.4
Fifth day (short) to sixth day (long)	2.4	1.7	5.8	1.7
Sixth day (long) to seventh day (short)	1.5	2.2	5.2	1.5
Seventh day (short) to eighth day (long)	—1.2	—1.7	2.9	—1.7

NOTE.—Table XX includes the records of 31 workers, all of whom were available for this part of the experiment though the work of some of them was not available for other parts of the experiment.

In the first place we may consider the improvement from day to day as measured from the median of one day's work to the median of the next (the first column of Table XX). In that case there is no advantage for the longer sittings. In fact, there seems to be a somewhat greater gain on the day following a short practice sitting. But this method of measurement involves an obvious error. The gain from a short day to a long day may be greater simply because of the gains made during the course of the long day, whereas the gain from a long to a short day may seem too small because there is not so good a chance to make a good record on the short day.

In order to avoid this difficulty a second mode of measurement has been pursued. This proceeds to consider only the first four trials of any day and in case there were more simply ignores them. We then have the improvement from day to day, but there is the additional factor that on certain days there were

four rounds of practice which were extra. The second column in the table shows that the improvement was greater after the days when there was extra practice. The extra practice also gives an advantage with regard to the possibility of making a single, sporadic record on the day following. These particularly good attempts reach a higher level on the days after a long day's practice than on the days after a short day's practice (third column in Table XX).

Finally, the last column of Table XX gives the answer to the question: Does the extra practice of the longer sitting result in an improvement on the following day beyond the gain which is made during the latter part of the day on which the prolonged practice occurs? In order to measure this we measure from the median of each day (whether long or short is not considered) to a definite point in the following day's work, namely, the end of four rounds. We measure so from the whole performance of one day to a fixed point (which is always reached) the following day. This method shows that the advantage of the extra practice in the longer sittings is used up during the course of the longer sitting itself and that the following day does not show a *further* improvement. The final conclusion seems to be that the extra practice of a longer sitting gives immediate results, but does not make an unusual gain possible on the following day.

Transmitted November 18, 1913.

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 5, pp. 323-344 March 3, 1916

DIURNAL VARIATIONS IN MEMORY AND ASSOCIATION

BY

ARTHUR I. GATES

I. The Problem and the General Method of Procedure

A year ago, the writer attempted to summarize the more important results obtained by earlier investigators on the problem of diurnal variations in efficiency, adding certain experimental findings of his own obtained by tests upon school children. A more extended discussion of the problem, its significance, and difficulties will be found in that article.[1]

The present investigation differs from the earlier one in several respects. In the present work adults served as subjects in the tests, which were extended over a greater portion of the day than previously. In the first investigation, moreover, tests for a variety of mental and motor functions were employed, while in the present work the tests are all included in the general field of memory and association.

One hundred and sixty-five college students, members of a class in elementary psychology, served as subjects for the experiments. Three full days were required to complete the experiments. The temperature on the particular days chosen varied from 56 to 66 degrees Fahrenheit. The tests were given to groups, each includ-

[1] Variation in Efficiency during the Day, together with Sex Differences, Practice Effects, and Correlations, *Univ. Calif. Publ. Psychol.*, vol. 2 (1916), no. 1.

ing from six to fourteen students at each hour of the day, except from noon to 1:00 o'clock, the first beginning at 8:00 A.M. and the last at 5:00 P.M. The subjects were seated at a long table, at one end of which stood the experimenter. The tests were given in the order in which the results are presented in this article. The time required for all the tests of any single sitting was about half an hour.

For the purpose of gaining fuller information with regard to his habits of life and especially of variations from the normal routine of the day, the following questionaire was filled out by each student:

1. Name.
2. College year.
3. Did you do any especially hard work today? What?
4. Did you do any especially hard work just before this hour? What?
5. What time do you usually go to bed?
6. What time do you usually get up?
7. Did the time of going to bed last night or the time of getting up this morning differ from the average? How much?
8. Do you usually eat a heavy or a light breakfast?
9. Do you usually eat a heavy or a light lunch?
10. Any exception today with regard to meals?
11. What hours do you think are your best for work or study?
12. What are your reasons for believing so?

The answers to question 7 showed that the majority of these students go to bed at 10:30 or 11:00 o'clock. The following table shows the distribution:

Hour	8:30	9:00	9:30	10:00	10:30	11:00	11:30	12:0
No. of inds.	1	8	24	43	71	64	22	7
Per cent	0.4	3.3	10.0	17.8	29.6	26.6	9.2	2.9

The following table shows the time of getting up:

Hour	5:00	5:30	6:00	6:30	7:00	7:30	8:00
No. of inds.	4	0	28	58	90	57	3
Per cent	1.6	0.0	11.6	24.2	37.4	23.7	1.2

Seven o'clock is the favorite hour of rising, although about one-fourth arise at 6:30 and another fourth at 7:30.

The matters of chief importance in this connection are the departures from the normal habits of life on the part of the sub-

jects. Answers to 2, 3, 7, and 10 gave the information with regard to such departures. In most cases where the subject had gone to bed much later than usual, had got up much later than usual, or missed a meal, or had some especially hard work just before the test, the data from him were not included.

Following are the chief factors, in addition to those just enumerated, which seemed to give just reason for discarding a subject's work: (1) Copying from others or using unfair methods of any sort, although such cases were very rare; (2) lack of interest, or inattention; (3) physical defects, such as conspicuous defects of sight or hearing, or sickness, bruised fingers, and the like, when of such a nature as to interfere with maximal performance; (4) failure to understand directions, etc., after the test had been started.

On the whole, however, nearly all of the subjects conformed to all the rules of the tests and the results were considered trustworthy and to be the products of their very best efforts. Many minor differences among the individuals existed with regard to the character of the day's activity. Some had many and some had few college exercises before the test; some had done more and some less studying than usual. The possibilities of such differences were, of course, greater for those coming in the latter part of the day; but except for reasons mentioned above, the data from all subjects were retained. Perfect conditions would consist in an identical programme for the day for all the subjects, one in which very little or no fatiguing work was done; but such conditions could not be obtained.

The following table shows the distribution of individuals according to their preferred hours for study and work:

	A.M.								NOON
Hour	4:00	5:00	6:00	7:00	8:00	9:00	10:00	11:00	12:00
No. of inds.	2	17	36	42	109	137	87	38	5
	P.M.								
Hour	1:00	2:00	3:00	4:00	5:00	6:00	7:00	8:00	9:00
No. of inds.	2	2	2	11	12	5	17	29	22
	10:00	11:00	12:00	No preference.					
	20	6	0	9					

The morning hours are preferred by most individuals, with the evening hours next in order. Nine A.M. is the most popular hour, although 8 A.M. is preferred by nearly as many persons. Ten A.M. follows next in order of preference and 6 and 7 A.M. rank higher than the best afternoon or evening hours. Eight P.M. is the preferred evening hour and 4 and 5 P.M. are the preferred afternoon hours.

The reasons given by the subjects for believing that their preferred hours are really their best hours are very much alike. In almost every instance the reasons amount merely to an opinion based on their subjective feelings. An effort will be made in this work to discover how closely the curve of distribution of preferred hours follows the curve of real efficiency as shown by the experimental findings. For the morning hours the reports are of this sort: "I feel more energetic;" "I am in better condition, physically and mentally;" "My mind is clearer;" and the like. Those who prefer the afternoon hours report: "I have got into the swing better;" "I am more wide awake;" and so on. The statements with regard to the evening hours are much the same, although those preferring the late evening and the very early morning hours attribute their greater efficiency to the existence of fewer distractions at those times. Many admit that they seem to have greater success at a particular time largely because, on account of the force of circumstances or by arbitrary choice, it had become habitual for them to do their hardest work at those hours.

II. Tests in Auditory Memory

For a test in auditory memory the following series of digits were used: 9627, 41852, 736294, 8513627, 38471629, 529468371, 2574638197, 83519472631, 628194357283

Detailed instructions with regard to the method of conducting the tests were given. The digits, beginning with the shortest series, were read sharply and without rhythm at the rate of one each three-fourths of a second, the rate being fixed by a silent

pendulum invisible to the subjects. The tests in auditory memory was given first of all to the groups on the first two days of the experiment, and second (following the visual series) on the last day.

The results are given in terms of the average (mean) spans with the mean variation, and of the median spans of the individuals in the different groups. By the "span" is meant the longest series reproduced correctly *in toto*. But in case a series

TABLE I. AUDITORY MEMORY

THE ABSOLUTE AND RELATIVE MEAN AND MEDIAN OF DIGITS REPRODUCED AT DIFFERENT HOURS

	A.M.				P.M.				
Hour	8:00	9:00	10:00	11:00	1:00	2:00	3:00	4:00	5:00
No. of subjects	23	23	28	28	25	25	25	30	25
Av. No. digits	7.81	7.61	7.72	8.08	7.60	7.38	7.67	7.46	7.33
Mean variation	1.20	1.10	1.15	1.05	1.40	1.07	0.92	1.00	1.10
Mean, per cent	100.0	97.5	98.8	103.3	97.4	94.5	98.2	95.5	93.8
Median	7.00	7.20	7.20	7.50	7.40	7.30	7.40	7.10	7.08
Median, per cent	100.0	103.0	103.0	107.0	105.8	103.9	105.8	101.4	101.0

Fig. 1.—Auditory memory.[2] The diurnal course of memory for auditory digits

[2] The diagrams are based on the percentage columns in the corresponding tables.

was correctly reproduced entire, following two or more incorrect series, the correct series preceding the failures was taken as the span. Thus, if an individual reproduced a series of six digits, failed at seven and eight, succeeded at nine and failed thereafter, the series of six digits was considered the span. The results are given in Table I.

The mean and the median both show a marked and steady increase in efficiency during the forenoon, with the exception that the mean for 8 o'clock is high compared to that for 9 or 10 o'clock. The median for this hour, however, is lower than for any other forenoon hour. The maximum efficiency for the day, according to both methods of computation, is at 11 o'clock. Efficiency at 1 o'clock is lower than at 11 o'clock and at 2 o'clock it is still lower, whence it moves upward to the afternoon maximum at 3 o'clock. According to the median, however, 3 P.M. is equal in efficiency to 1 P.M.; and although the mean at 1 P.M. is less than at 3, it is greater than at 2 o'clock. It will be noticed, however, that the mean variation is greater at 1 o'clock than at any other hour of the day, indicating the small reliability of this measure. In general, there is a steady and pronounced increase in efficiency during the forenoon, a fall following the lunch hour, an increase at 3 P.M., and a final drop at the end of the day.

III. TESTS IN VISUAL MEMORY

The following series of digits were used as tests of visual memory: 6283, 57294, 241738, 2170463, 27986543, 215903847, 5978024318, 57402623871, 183570467392. The displays were made up of black gummed digits two and three-fourths inches in height, pasted on white cards two and a half by three and a half inches in size, which were pasted in series on a strip of gray cloth.

The strips were exposed in order of length, beginning with the series of four digits. The folded strip was held before the subjects, the word "ready" was given and the strip was drawn taut with a snap, exposing the series. The subjects wrote the digits as soon as the strip was taken from sight. The time of exposure

varied with the length of the series, being determined by multiplying the number of digits in each series by three-fourths of a second.

The method of treating the data and computing the results was the same as that employed in the tests of auditory memory. The results are stated in the absolute and relative number of digits reproduced, determined by the arithmetical mean and median.

TABLE II. VISUAL MEMORY

THE ABSOLUTE AND RELATIVE MEAN AND MEDIAN OF DIGITS REPRODUCED AT DIFFERENT HOURS

	A.M.				P.M.				
Hour	8:00	9:00	10:00	11:00	1:00	2:00	3:00	4:00	5:00
No. of subjects	23	24	28	24	24	25	26	26	25
Av. No. digits	8.00	7.95	8.14	8.14	7.84	8.08	8.15	8.17	8.00
Mean variation	1.25	1.13	1.06	1.19	1.20	1.30	1.08	1.16	1.20
Mean, per cent	100.0	99.3	101.5	101.5	98.0	100.1	101.7	102.0	100.0
Median	7.50	7.50	7.70	7.60	7.25	7.50	7.80	7.60	7.50
Median, per cent	100.0	100.0	103.0	101.5	96.7	100.0	104.0	101.5	100.0

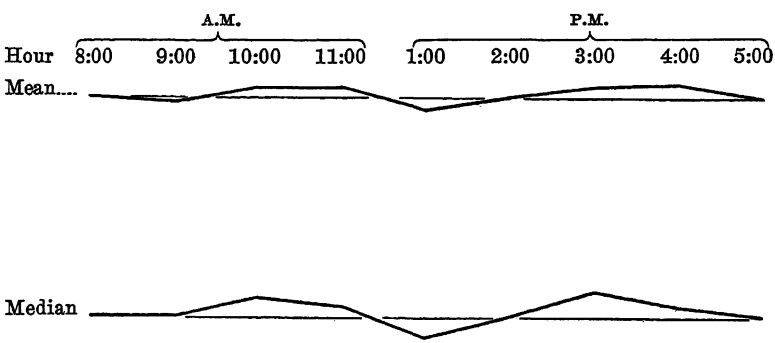


Fig. 2.—The diurnal course of visual memory

In many respects the course of efficiency in memory for visual digits is the same as for auditory digits. The general increase in efficiency during the forenoon is evident, although the same

exception is found with regard to the 8 o'clock hour—i.e., superiority of that hour over 9 o'clock, as shown by the mean bu not by the median. In the case of visual memory, howeve 11 A.M. is barely equal to 10 A.M. according to the mean, and slightly inferior according to the median. In the afternoon, fro the fall at 1 P.M. there is a steady increase in efficiency unt 3 P.M., as shown by the median; and the mean indicates that th increase persists until 4 P.M. Both measures show a decrease i efficiency following 4 o'clock. On the whole, there is a high degre of correspondence between the curves for the two forms memory for digits.

IV. Substitution Test

For a test in the rapidity of learning, a form of code w used.[a] This test gives a fair measure of the rapidity with whi associations are formed by repetition. The associations consist the formation of connections between a series of nine symb and nine digits, and are established gradually as the test procee

The material consisted of printed cardboard "key" form three by seven inches, containing nine circles, in each of whi was enclosed the symbol and the corresponding digit; and al of test sheets eight and a half by eleven inches in size, on ea of which were printed two columns of twenty-five rows of fi symbols each, with blank spaces to correspond. Altogether, ea sheet contained 250 symbols and 250 blank spaces.

The test sheets and the keys were passed to the subjects, t keys face downward and the test sheets face upward in pro position. The method of conducting the test was then explai in detail. Six minutes were allowed for the work.

The test sheets were checked up for errors and, excludi them, the total number of digits written was computed. T results are given in Table III.

Again an increase in efficiency during the forenoon is fou although 11 o'clock is slightly inferior to 10 o'clock. The aft

[a] For a discussion of this test, see Whipple, G. M., *Manual of Phy and Mental Tests* (1910), pp. 350 ff.

noon maximum stretches from 2 to 4 o'clock, the figures for these hours being nearly the same. The hours of lowest efficiency for the day are 1 P.M. and 5 P.M., the latter being slightly below the former. The curve, in a general way, is very closely akin to those found for the previous functions.

TABLE III. SUBSTITUTION TEST

THE ABSOLUTE AND RELATIVE MEAN AND MEDIAN FOR DIFFERENT HOURS

	A.M.				P.M.				
Hour	8:25	9:25	10:25	11:25	1:25	2:25	3:25	4:25	5:25
No. of subjects	18	23	25	23	19	23	27	24	21
Av. No. digits	174.0	179.0	183.4	181.8	167.0	179.0	177.0	176.6	164.0
Mean variation	33	30	31	27	30	31	33	34	31
Mean, per cent	100.0	102.7	105.2	104.3	96.0	102.6	101.5	101.2	94.3
Median	174.0	176.0	181.0	180.0	166.0	169.0	170.0	170.0	165.5
Median, per cent	100.0	101.2	104.0	103.4	95.5	97.2	97.7	97.7	95.3

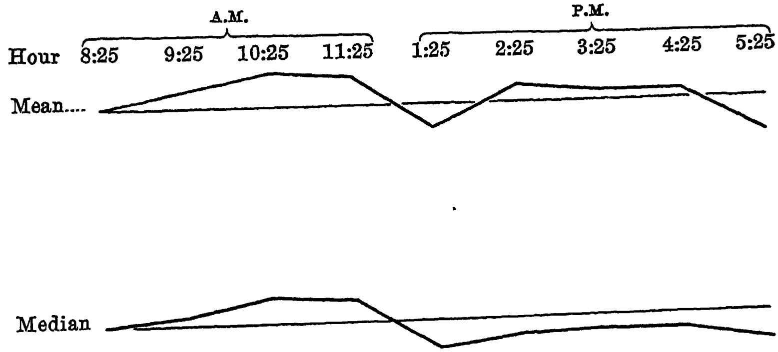


Fig. 3.—The diurnal course of efficiency in the substitution test

V. Tests in Recognition

The material consisted of a sheet of twenty-five geometrical figures for a learning series and a larger sheet containing the original twenty-five figures mixed indiscriminately with twenty-five new figures for a test series. The subjects marked off on the

test sheet the recognized figures. No one succeeded in recognizing all of the figures, and very few completed the tests without erroneous recognitions.

The method of conducting the test was explained in detail and both sheets were passed out face downward. One minute was allowed for the study period and two minutes for the recognition work.

TABLE IV. RECOGNITION TEST

THE ABSOLUTE AND RELATIVE MEAN AND MEDIAN OF FIGURES CORRECTLY RECOGNIZED

	A.M.				P.M.				
Hour	8:30	9:30	10:30	11:30	1:30	2:30	3:30	4:30	5:30
No. of subjects	21	24	29	28	24	25	27	29	22
Av. No. recognized	13.1	14.5	15.1	14.3	13.6	14.0	15.3	16.3	16.3
Mean variation	2.5	2.8	2.7	2.0	2.1	2.3	2.2	3.1	3.3
Mean, per cent	100.0	111.5	115.0	109.0	104.0	107.0	116.8	124.5	124.5
Median	11.7	15.0	15.0	14.5	12.5	14.2	15.2	16.1	16.0
Median, per cent	100.0	128.0	128.0	124.0	107.0	121.0	130.0	137.5	136.6

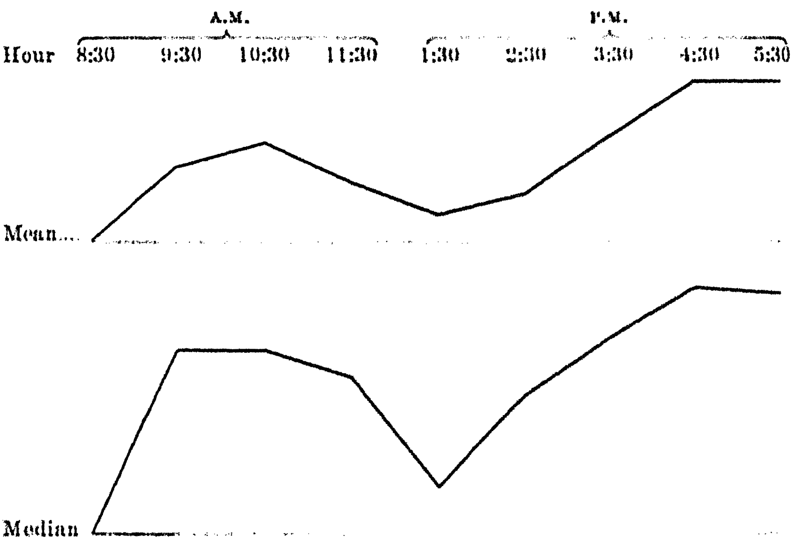


Fig. 4.—The diurnal course of efficiency for recognition of figures

The data were scored by counting the number of figures correctly cancelled and the number incorrectly cancelled. Twenty-five would be a perfect score. The results have been computed, first, on the basis of the number of figures correctly cancelled; second, on the basis of a penalized score in which, from the number correctly recognized, one credit is deducted for each erroneous recognition; and finally, tabulations have been made of the number of errors occurring at each hour.

Table IV gives the results according to the first method of computation.

The main features of the curves for efficiency in the recognition of figures are the same as those found for the functions previously considered. The differences from hour to hour, especially those chosen by the median, are exceedingly great, the relative differences being here many times greater than those found in the memory tests. Both mean and median agree in showing a large and steady increase during the forenoon, which has ceased, however, by 11:30 A.M. Compared to the efficiency of the morning hours, a decided drop is evident at 1:30 P.M., which is followed by a steady increase until 4:30 P.M.; 5:30 P.M., although lower on the curve than 4:30 P.M., is still very high, but it will be noted that the mean variations for the last hours are very high. On the whole, there is a high correspondence between the general form of the curves for this function and those previously considered.

Table V gives the results when, from the scores just considered, one credit was deducted for each erroneous recognition. It is very probable that this arbitrary method may give a good index of the real efficiency of many individuals. Some subjects work cautiously, making but few erroneous judgments. Others are less careful, marking out almost any figure which seems at all familiar; and, by increasing the number of cancellations, mark out a greater number of the proper figures, in many cases by mere chance, since the numbers of right and wrong figures were equal. The arbitrary penalty has for its basis the fact of equal distribution of right and wrong figures, and one credit is therefore deducted for each error.

TABLE V

THE ABSOLUTE AND RELATIVE MEAN AND MEDIAN OF FIGURES RECOGNIZED (PENALIZED SCORE)

	A.M.				P.M.				
Hour	8:30	9:30	10:30	11:30	1:30	2:30	3:30	4:30	5:30
Av. No. recognized	10.8	12.5	13.2	12.5	11.5	12.0	13.0	13.0	12.8
Mean variation	2.0	1.9	1.9	1.8	1.9	1.8	1.8	2.1	2.0
Mean, per cent	100.0	115.7	122.2	115.7	106.5	111.0	120.0	120.0	118.5
Median	11.1	12.0	13.0	12.3	10.6	11.0	13.0	13.5	13.0
Median, per cent	100.0	108.0	117.0	112.0	96.4	99.0	117.0	121.0	117.0

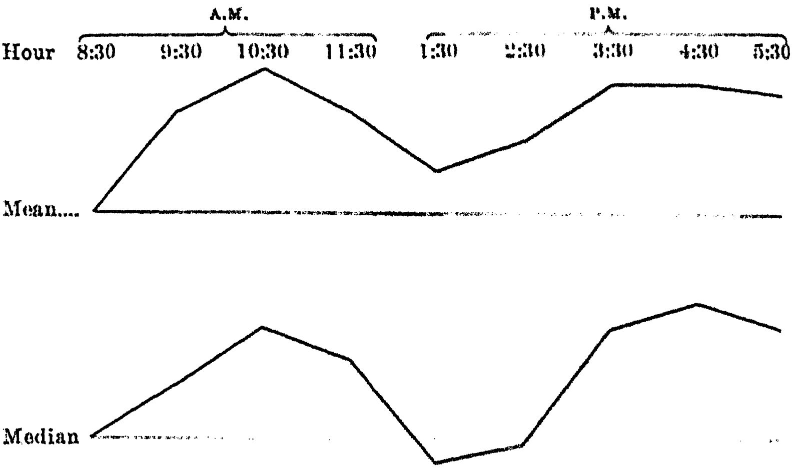


Fig. 5.—The diurnal course of recognition of figures. (Penalized score)

The figures, relatively, are not greatly changed by penalizing the subjects for erroneous recognitions. 11:30 A.M., by virtue of fewer errors, shows a slightly higher efficiency, while 5:30 P.M., on account of more frequent errors, shows less relative efficiency than before. The mean variations of the two last hours of the day remain high. The changes brought about by penalizing the subjects for errors have resulted in making the curves correspond more closely than formerly to the curves of functions considered earlier.

Table VI shows the amount of errors made at different hours of the day, without regard to the total amount of correct recognitions. The percentage columns show efficiency and do not show the relative number of errors; i.e., the larger this percentage, the fewer the errors.[4]

TABLE VI. ERRORS IN RECOGNITION

SHOWING THE MEAN AND MEDIAN ERRORS MADE, WITH THE RELATIVE EFFICIENCY AT EACH HOUR

	A.M.				P.M.				
Hour	8:30	9:30	10:30	11:30	1:30	2:30	3:30	4:30	5:30
Av. No. errors	2.31	2.04	1.78	1.93	2.16	2.40	1.90	3.06	2.50
Mean variation	1.13	1.40	1.22	1.76	1.45	1.48	1.27	1.82	1.95
Relative efficiency	100.0	112.0	130.0	120.0	107.0	96.3	121.5	75.5	92.5
Median	2.00	2.00	1.50	1.50	1.80	2.15	1.32	2.30	2.00
Relative Median	100.0	100.0	133.0	133.0	111.0	93.0	151.5	87.0	100.0

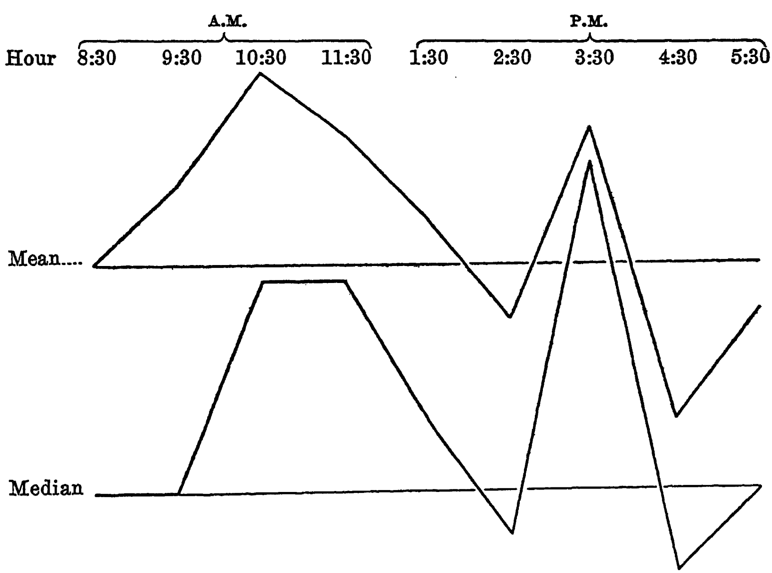


Fig. 6.—The diurnal course of efficiency with regard to decrease of erroneous recognitions

[4] The figures showing the relative efficiency are obtained by dividing the score for eight o'clock by the score for any particular hour.

The number of errors made seems to decrease during the forenoon, the fewest being made at 10:30 A.M., with 11:30 A.M. next in order. During the afternoon the smallest number of errors are made at 3:30 P.M., with a great many in the 2:30 P.M. hour. Although the results are very irregular and the mean variations exceedingly high, there is a slight indication that the hours at which the smallest number of errors were made were the hours of greatest efficiency otherwise.

VI. Tests in Logical Memory[5]

Tests in logical memory, or memory for ideas, have long been used to determine individual differences in mnemonic efficiency, as related to age, sex, training, etc. This test differs from the preceding tests of memory in two respects; first, material which forms a significant whole is used instead of a series of disconnected impressions; and second, the reproduction of ideas rather than the exact reproduction of sensory forms is required. Therefore, this is called a test of *logical* memory, in contrast to *rote* memory.

One of the familiar texts devised by Healy was used, beginning "If a man finds that his house is on fire, he should look to see if it is a large fire, etc."[6] The material is divided into twenty parts, each of which is counted one detail, or "idea." The method of conducting the test was carefully explained. The material was read slowly, and ample time was allowed in which to write the details remembered. One unit of credit was given for the recall of each "idea"; thus an accurate reproduction of all the ideas would give a score of twenty. The results are given in Table VII.

The results of this test for logical memory are quite irregular; mean and median do not harmonize as closely as usual. The increase in efficiency during the forenoon which has generally been found to be the rule, does not appear clearly in this func-

[5] This test was conducted by Miss Ida A. Felt, a graduate student in psychology, who used the data for other purposes.

[6] See Healy, W., and Fernald, G. M., Tests for Practical Mental Classifications, *Psych. Monog.* (1911), no. 54.

TABLE VII. LOGICAL MEMORY

THE ABSOLUTE MEAN AND MEDIAN OF THE NUMBER OF IDEAS REPRODUCED

	A.M.				P.M.				
Hour	8:40	9:40	10:40	11:40	1:40	2:40	3:40	4:40	5:40
No. of subjects	22	23	27	28	22	22	25	28	26
Av. No. "ideas"	13.20	14.40	14.20	13.60	12.60	13.10	13.40	13.50	12.05
Mean variation	2.2	1.4	2.4	2.6	2.4	1.9	2.1	1.8	1.5
Mean, per cent	100.0	109.0	107.7	103.0	95.5	99.3	101.4	102.2	91.3
Median	13.5	14.5	14.0	14.2	13.5	13.5	14.0	13.5	12.6
Median, per cent	100.0	107.3	103.7	105.1	100.0	100.0	103.7	100.0	93.3

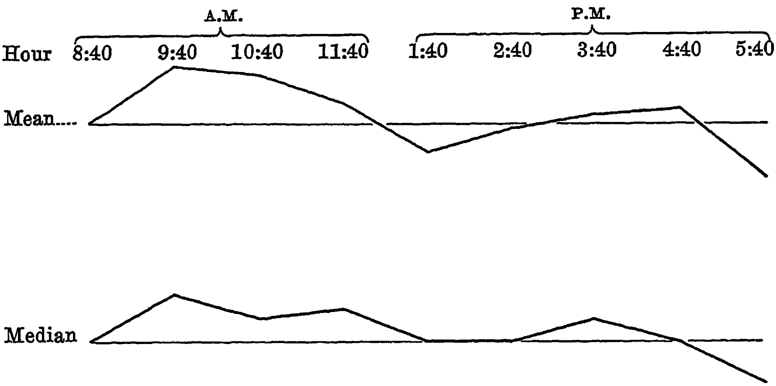


Fig. 7.—The diurnal course of efficiency for logical memory

tion. The maximum of the day appears early, at 9:40 A.M. The mean indicates a superiority of 10:40 over 11:40 A.M., but the median shows the latter hour to be superior. It will be noted that the mean variations for these hours are very large. More uniform and more characteristic results appear in the afternoon curves. The mean shows an increase from a very low efficiency at 1:40 P.M. to an afternoon maximum at 4:40. The latter hour is, however, but very slightly superior to 3:40 P.M. The median indicates a maximum for the afternoon at 3:40 o'clock. The final decrease in efficiency at 5:40 is clear in both cases. The curves

for the afternoon hours, then, conform fairly closely to the curves found in the case of most other functions previously considered, but with regard to the forenoon hours the results are not harmonious.

VII. Summary of Results

The accompanying table and figures show the curves for the various functions that have been considered, grouped for purposes of comparison. The mean and median of each function are given as percentages, and a final average for the mean and median figures of all functions is given.

TABLE IX. ALL FUNCTIONS

The Mean and Median, per cent, for the Various Functions

	A.M.				P.M.				
Hour*........	8:00	9:00	10:00	11:00	1:00	2:00	3:00	4:00	5:00
Auditory memory.									
Mean	100.0	97.5	98.8	103.3	97.4	94.5	98.2	95.5	93.8
Median	100.0	103.0	103.0	107.0	105.8	103.9	105.8	101.4	101.0
Visual memory.									
Mean	100.0	99.3	101.5	101.5	98.0	100.1	101.7	102.0	100.0
Median	100.0	100.0	103.0	101.5	96.7	100.0	104.0	101.5	100.0
Substitution.									
Mean	100.0	102.7	105.2	104.3	96.0	102.6	101.5	101.2	94.3
Median	100.0	101.2	104.0	103.4	95.5	97.2	97.7	97.7	95.3
Recognition. *(Penalized score)*									
Mean	100.0	115.7	122.2	115.7	106.5	111.0	120.0	120.0	118.5
Median	100.0	108.0	117.0	112.0	96.4	99.0	117.0	121.0	117.0
Logical memory.									
Mean	100.0	109.0	107.7	103.0	95.5	99.3	101.4	102.2	91.3
Median	100.0	107.3	103.7	105.1	100.0	100.0	103.7	100.0	93.3
Average†	100.0	104.3	106.6	105.6	98.7	100.6	105.1	104.2	100.4

* The hours at which the series of tests were begun is given here. The actual time at which each test was given can be obtained from Tables I to VIII above.

† Average of means and medians of all functions.

The existence of many irregularities and fluctuations in the various curves is at once evident. Yet when one overlooks these minor differences, it can be seen with equal clearness that there

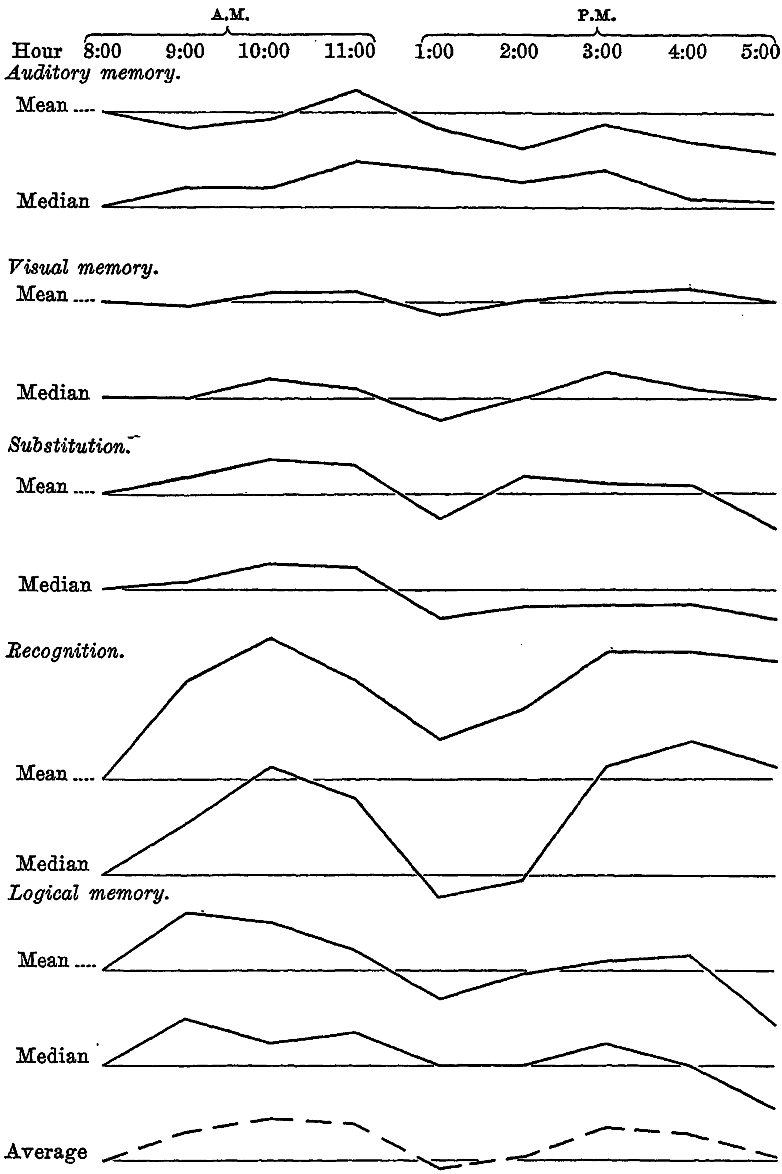


Fig. 8.—All functions. Curves based on Table IX, showing the course of efficiency for all functions

is throughout a great similarity in the general form of the curves. The average curve shows the characteristic trend of all those of which it is made up. Beginning at 8 A.M. the curve moves steadily upward until it reaches a maximum between 10 and 11 o'clock. In the average curve, 10 o'clock surpasses 11 o'clock slightly; a fact which is shown by some of the individual curves, while others show the opposite. Compared to the later afternoon hours, 1 P.M., in the average figure, is very low; in fact, it is the time of minimum efficiency of the day. All of the individual curves agree in showing 1 o'clock to be a poor hour. From the 1 o'clock minimum, the average curve moves upward to an afternoon maximum at 3 o'clock and then tends downward again until the last hour of the day, which shows an efficiency about equal of that of the first morning hour. There are many departures from the central tendency; some curves, for example, show an afternoon maximum at 3 o'clock, others at 4, and in some cases 2 P.M. stands very high; but all show more or less definitely the characteristic wave of efficiency.

In an earlier section (p. 325), a table was made showing the distribution of the individuals taking part in the experiment according to their preferred hours for study. A part of the table is reproduced here.

	A.M.					
Hour	6:00	7:00	8:00	9:00	10:00	11:00
Inds.	36	42	109	137	87	38
	P.M.					
Hour	1:00	2:00	3:00	4:00	5:00	6:00
Inds.	5	2	2	11	12	5

In the light of the results we have just obtained, it appears that the subjects' estimates of their best hours are very often incorrect. Too few chose the best hours, 10 or 11 A.M.; too many chose too early an hour; and very few appreciate the efficiency of the afternoon hours, noticeably 3 o'clock. The conclusion is that one's subjective feelings with regard to the time of greatest efficiency are not reliable indications of real efficiency. The

organism may be able to produce the greatest amount and the best quality of work at a time when feelings of fatigue and kindred factors lead us to believe that our efficiency is low.

VIII. Comparison with Results Obtained by Other Investigators

The literature of the subject of diurnal variations in efficiency can be found in summarized form in two monographs.[7] But briefest mention can be made here of a few of the more extensive pieces of work.

Marsh, in tests of arithmetical ability, memory, attention, and perception, found that, as a rule, the mid-day periods (12:00–2:00 P.M.) were superior to the afternoon periods (4:00–7:00 P.M.), which were superior to the morning periods (7:00–9:00 A.M.). Winch[8] found memory better in the late forenoon (9:45–10:05 A.M.) than in the afternoon (4:00–4:20 P.M.); and better in the afternoon than in the early morning. Robinson,[9] in arithmetical work, found a rise in efficiency during the forenoon, culminating at 10:30 A.M., with a noticeable drop at 12:30 P.M., and a subsequent rise until 2:00 P.M. Hollingworth,[10] in tests for various functions, found a low efficiency at the beginning of work, then a gradual increase followed by a period of low efficiency. Hollingworth's results and his interpretation of them are not in accord with the present findings. The present writer, in his earlier study,[11] obtained results from tests on school children which agree in most respects with the present findings.

[7] Marsh, H. D., The Diurnal Course of Efficiency, *Colum. Univ. Contr. Philos.*, vol. 14 (1906), no. 3. Gates, A. I., Variation in Efficiency during the Day, etc., *Univ. Calif. Publ. Psychol.*, vol. 2 (1916), no. 1.

[8] Winch, W. H., Mental Fatigue in Day-School Children as Measured by Immediate Memory, *Journ. Ed. Psych.*, vol. 3 (1912), pp. 18–29, 75–82; Mental Adaptation during the School Day, *Journ. Ed. Psych.*, vol. 4 (1913), pp. 17–28, 71–84.

[9] Robinson, L. A., Mental Fatigue and School Efficiency, *Publ. Winthrop Norm. and Indus. Coll. S. C.*, vol. 5 (1912), no. 5.

[10] Hollingsworth, H. L., Variations in Efficiency during the Working Day, *Psych. Rev.*, vol. 21 (1914), pp. 473–492.

[11] *Loc. cit.*

The curve of efficiency found by Robinson, characterized by an increase during the forenoon, a fall following the lunch hour and a subsequent increase in the afternoon, is borne out by the present findings, as are also the results of Marsh, showing the superiority of the mid-day period over afternoon and morning periods, and Winch's results, showing a relatively great efficiency in the late forenoon. The present piece of work has produced no evidence in support of Hollingworth's results.[12] However, when heed is given to numerous factors, such as climate, habits of life, the character of tests employed, the methods of testing, and so on, which may differ with the different investigations, it is not surprising that they should lack entire harmony. On the contrary, the degree of uniformity which does exist is surprising.

IX. Conclusions

The facts that have been brought forth by this study indicate the existence of a diurnal course of efficiency in an important type of mental activity. From the early morning the efficiency of the organism begins to increase, culminating in a maximum in the late forenoon, followed by a decline immediately after the noon meal, with a subsequent rise until the middle of the afternoon, or a little later, and a final drop in the late afternoon.

Such is the broad rhythm found with fair regularity for groups of individuals. But it must be admitted that within this general trend exist numerous variations among the individuals. Differences in daily habits of life, in the physical and mental make-up, and in external conditions account, in a large measure, for such variations. Each individual, to profit most thoroughly from the knowledge of the nature of his daily rhythm of efficiency, should determine it by experimental methods for himself.

The demonstration of such a diurnal rhythm carries with it the suggestion that psychological and physiological investigators

[12] Hollingworth's results have been discussed in the writer's earlier paper.

should take into consideration more carefully than heretofore the fact of periodicity in individuals. The results of experiments conducted at different hours of the day are in danger of being disturbed by large errors due to diurnal fluctuations. One who reads the literature of years of investigation for the purpose of measuring mental fatigue can readily see the possibility that diurnal variations in efficiency have probably been a prominent cause of conflict in the findings.

Yet the application of the fact of diurnal rhythms to the problem of fatigue is difficult. This is due to the fact that the curve of efficiency, as we have found it, is influenced by the effects of fatigue, incitement, adaptation, and other factors. The subjects working at 10 A.M. in our experiments were doubtless laboring under greater fatigue than those who worked at 8 A.M., yet in spite of greater fatigue their efficiency was higher. If they had been equally fresh, their efficiency might have been considerably higher still. Just what part fatigue, feelings of fatigue, lack of interest, adaptation and such factors play, it is impossible to say.

The fact is significant that the hours of greatest efficiency are those at which fatigue, it would seem, should be very great. We have found in the present study, moreover, that most persons, basing their judgments largely on their subjective feelings, did not select as their best hours for study those which actually stood highest in efficiency according to the tests. Thorndike,[13] in summarizing the investigations bearing on this point, concludes: "The feelings of fatigue, from what we know of them, thus seem to be a very poor symptom of the loss of ability." An opportunity is afforded us to profit by such information. The feelings of weariness which become more insistent as we indulge them might be made more and more to disappear, provided the organism is in healthy condition, if we work on in spite of them. A man must be trained to meet situations where he must put forth more than the customary amount of effort. He should, by practice in the voluntary disregard of mere feelings of fatigue, learn the limits

[13] Thorndike, E. L., *Educational Psychology*, vol. 3, part 1, p. 107.

of his ability and by so doing attain greater assurance, and, by virtue of this, greater efficiency.

The drop in efficiency following the mid-day meal has been the most consistent aspect of the curve that has been found. No exception to this rule occurs in the case of any function, and the average results show that the minimum efficiency of the day occurs at this hour. Since the returns for labor at this time are relatively so small, it would seem that an hour or two, following the noon meal, could be wisely spent for rest or recreation. *Plenus venter non studet libenter* is a maxim to which the student should give more heed. Such was Offner's firm conviction when he wrote:[14] "It is, accordingly, one of the most justifiable demands of school hygiene that the afternoon session . . . should begin, at the very least, two hours after the noon meal, i.e., at 3 o'clock and not at 2 o'clock." Such an implication is not altogether clear, however, because the exact causes of diminished efficiency at this time cannot be accurately determined. The physiological effects of the mid-day meal are, perhaps, not the only influences at work. It may well be that relaxation or mere cessation of work causes the state of adaptation, attained during the forenoon exercise, to subside; and high efficiency can again be attained only by means of another process of "warming up." If such is the case, the exact time at which work is renewed would not be so important a consideration. It was found, moreover, in the earlier investigation,[15] that efficiency in functions of the motor type was quite high soon after lunch. Such school subjects as drawing, painting, modeling, writing, manual arts and the like might be profitably pursued during the early afternoon hours.

[14] Offner, M., *Mental Fatigue*, translated by Whipple (1911), pp. 88–89.
[15] Variations in Efficiency, etc., *Univ. Calif. Publ. Psychol.*, vol. 2 (1916).

Transmitted October 28, 1915.

UNIVERSITY OF CALIFORNIA PUBLICATIONS
IN
PSYCHOLOGY

Vol. 1, No. 6, pp. 345-350 May 2, 1916

CORRELATIONS AND SEX DIFFERENCES IN MEMORY AND SUBSTITUTION

BY
ARTHUR I. GATES

One hundred and ninety-seven students, members of a class in elementary psychology, were tested during the academic year 1914–15 for memory of visual and auditory digits, memory of verbal sense material, recognition of geometrical figures, and learning in a substitution (code) test. The results of the experiment were first used for other purposes. This paper presents the correlations of these tests, together with the distribution of the individuals and sex differences in the several functions.

The material and method of procedure have been described elsewhere[1] and will be given here only in brief. Three full days were required to complete the experiments. The tests were given to groups, each including from six to fourteen subjects, at each hour of the day, the first beginning at 8 A.M. and the last at 5 P.M., with exception of the hour from noon to 1 P.M. The time required for all the tests at any single sitting was about half an hour. The tests were given in the following order:

1. *Auditory digits.* Eight series, of from four to twelve digits each, were read successively at the rate of one each three-fourths of a second, beginning at the shortest series. Computations are based on the "span" of each individual.

2. *Visual digits.* Eight similar series were prepared on strips of cloth, each series being exposed as a whole, the time of ex-

[1] Diurnal Variations in Memory and Association, *Univ. Calif. Publ. Psychol.*, vol. 1 (1916), pp. 323–344.

posure being determined by multiplying the number of digits in the series by three-fourths of a second. The computations are based on the "span."

3. *Substitution (code) test.* A modification of Dearborn's code test containing 250 symbols was used. Six minutes were allowed for the test. Computations are based on the number of symbols correctly transliterated.

4. *Recognition of forms.* Twenty-five geometrical figures were studied one minute, followed by a two-minute test, during which the recognized forms were cancelled from a second sheet containing the original and twenty-five new figures.

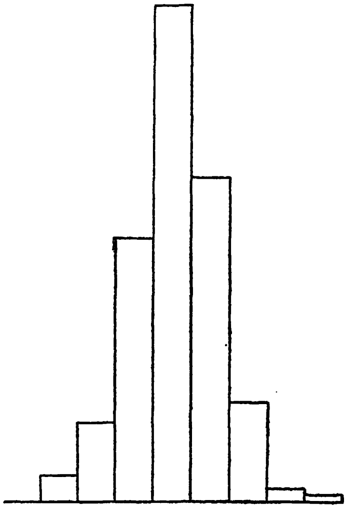
5. *Verbal memory.* The "House on Fire" text was read. Computations show the number of details remembered.

1. Distribution of Individuals

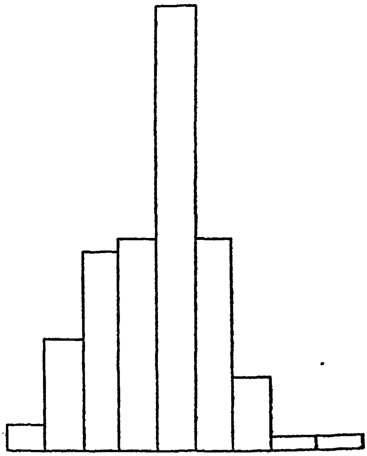
Table I shows the distribution of the individuals in the several functions.

TABLE I

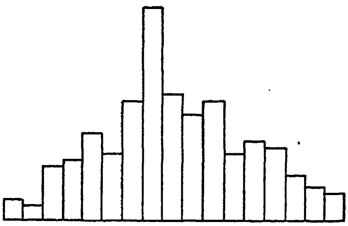
Auditory Memory (digits)		Visual Memory (digits)		Substitution		Recognition		Auditory Memory (connected ideas)	
No. of digits	No. of indvs.	No. of digits	No. of indvs.	No. of symbols	No. of indvs.	No. of figs.	No. of indvs.	No. of ideas	No. of indvs.
12	2	12	1	260 250	4	22	1	19	7
11	2	11	2	250 240	5	21	5	18	12
10	11	10	15	240 230	7	20	2	17	10
9	32	9	49	230 220	11	19	7	16	22
8	67	8	75	220 210	12	18	5	15	27
7	32	7	39	210 200	10	17	13	14	26
6	30	6	12	200 190	18	16	21	13	20
5	17	5	4	190 180	16	15	20	12	23
4	4	4	0	180 170	19	14	19	11	14
				170 160	32	13	17	10	10
Total, 197		Total, 197		160 150	18	12	23	9	12
				150 140	10	11	23	8	3
				140 130	13	10	17	7	3
				130 120	9	9	10	6	4
				120 110	8	8	3	5	1
				110 100	2	7	4	4	2
				100 90	3	6	4	3	1
						5	1		
				Total, 197		4	2	Total, 197	
						Total, 197			



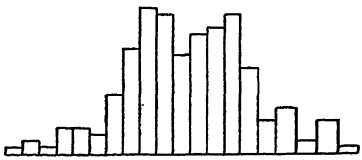
A



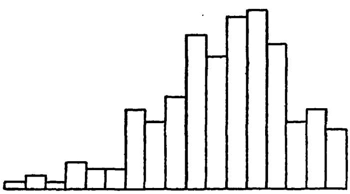
B



C



D



E

The graphs A–E are based on Table 1, the scales reading from left to right representing the ranges from least to greatest abilities. The vertical heights represent the number of individuals. A. Auditory Memory (digits). B. Visual Memory (digits). C. Substitution. D. Recognition. E. Auditory Memory (connected ideas).

Figure 1 shows graphically the distribution of individuals. On the whole the figures do not differ greatly from the "normal" or "chance" form of distribution. Figures A and B, for auditory and visual memory respectively, approximate the "normal" distribution quite closely. Figure C, for the substitution test, is fairly regular, and likewise figure D, for the recognition test, except for the depression just where the mode would be expected. Figure E, for auditory memory of verbal sense material, is skewed toward the upper end.

2. Correlation of Tests

Coefficients of correlation were computed by means of the familiar formula:

$$r = 1 - \frac{6\,\Sigma D^2}{n(n^2 - 1)}$$

Table II gives the correlations for the 197 subjects.

TABLE II

	Visual Memory (digits)	Auditory Memory (digits)	Auditory Memory (ideas)	Recognition	Substitution
Visual memory (digits)	....	+0.41	+0.07	+0.21	+0.19
Auditory memory (digits)	+0.41	....	+0.25	+0.25	+0.26
Aud. memory (connected ideas)	+0.07	+0.25	....	+0.09	+0.28
Recognition	+0.21	+0.25	+0.09	....	+0.34
Substitution	+0.19	+0.26	+0.28	+0.34	....
Average	+0.22	+0.29	+0.16	+0.22	+0.26

The correlations are all positive, but are for the most part low. The highest coefficient +0.41, existing between visual and auditory memory, might be expected. The coefficient of +0.34 between recognition and the code test is next in order of magnitude, followed by +0.28 between verbal memory and the code test. The lowest correlations are +0.07 and +0.09, the former between verbal and visual memory and the latter between verbal memory and recognition. On the whole the correlations with the auditory memory test were highest (average +0.29), closely followed by the code test (average +0.26), with the recognition

test and the visual memory test (average +0.22 in each case) next in order, and lastly the verbal memory test, whose average coefficient was but +0.16.

3. Correlations for the Sexes Separately

Table III shows the correlations for the two sexes taken separately.

TABLE III

63 Men

	Visual Memory (digits)	Auditory Memory (digits)	Auditory Memory (ideas)	Recognition	Substitution
Visual memory (digits)	....	+0.43	+0.06	+0.29	+0.06
Auditory memory (digits)	+0.43	....	+0.10	+0.42	+0.20
Aud. memory (connected ideas)	+0.06	+0.10	....	+0.10	+0.27
Recognition	+0.29	+0.42	+0.10	....	+0.42
Substitution	+0.06	+0.20	+0.27	+0.40	....
Average	+0.21	+0.29	+0.18	+0.32	+0.23

134 Women

	Visual Memory (digits)	Auditory Memory (digits)	Auditory Memory (ideas)	Recognition	Substitution
Visual memory (digits)	....	+0.40	+0.03	+0.14	+0.13
Auditory memory (digits)	+0.40	....	+0.10	+0.20	+0.17
Aud. memory (connected ideas)	+0.03	+0.10	....	+0.06	+0.22
Recognition	+0.14	+0.20	+0.06	....	+0.28
Substitution	+0.13	+0.17	+0.22	+0.28	....
Average	+0.17	+0.22	+0.10	+0.17	+0.20

The separate correlations for the sexes are strikingly similar. The coefficient between visual and auditory memory is highest for both sexes and that for verbal memory and visual digits is lowest. A Pearson coefficient of +0.54 exists between the order of these single correlations for the sexes. For the women, the auditory digit test has the highest average correlation with all others, while in the case of the men the recognition test leads. Verbal memory has the lowest average correlation for both sexes. A Pearson coefficient of +0.41 is found between these average correlations for the sexes.

The correlations of the men are larger in nearly every case, the only exceptions being the correlation between substitution and visual digits, in which the women show the larger coefficient, and the correlation between verbal memory and auditory digits, in which the sexes are equal.

4. SEX DIFFERENCES IN EFFICIENCY

Table IV gives the mean score obtained by the sexes separately and a relative score using a score of 100.0 for the women as a basis.

TABLE IV

	Score of 134 women	Score of 63 men	Relative score, men
Visual memory (digits)	8.107	8.0	98.6
Auditory memory (digits)	7.54	7.48	99.7
Auditory memory (connected ideas)	13.7	12.6	91.9
Recognition	12.7	12.8	100.7
Substitution	172.8	178.9	103.5

The results, so far as the memory tests are concerned, are in harmony with the generally accepted belief, i.e., that women excel in this kind of work. The amounts by which the women excel are small, except in the case of verbal memory, in which they surpass the men by 8.1 per cent. The men excel slightly in the substitution test, but the difference is not great.

5. SUMMARY

1. A fairly uniform distribution of individuals is found for the several functions.
2. Positive correlations ranging from +0.07 to +0.41 are found between the functions.
3. The correlations for the sexes separately are very similar, although those for the men are higher in nearly every case.
4. The women excel slightly in the memory tests, while the men are somewhat superior in the substitution test.

Transmitted February 26, 1916.

11. A Theory of the Syllogism, by Knight Dunlap. Pp. 227-235 .... .10
12. The Basal Principle of Truth-Evaluation, by Harry Allen Overstreet. Pp. 236-262 .... .25

Vol. 2. 1. The Dialectic of Plotinus, by H. A. Overstreet. Pp. 1-29. May, 1909.... .25
2. Two Extensions of the Use of Graphs in Formal Logic, by W. E. Hocking. Pp. 31-44. May, 1909 .... .15
3. On the Law of History, by W. E. Hocking. Pp. 45-65. September, 1909 .20
4. The Mystical Element in Hegel's Early Theological Writings, by George Plimpton Adams. Pp. 67-102. September, 1910 .... .35
5. The Metaphysics of Historical Knowledge, by DeWitt H. Parker. Pp. 103-186. February, 1913 .... .85

ERICAN ARCHAEOLOGY AND ETHNOLOGY.—Alfred L. Kroeber, Editor. Price per volume $3.50 (volume I, $4.25). Volumes I-X completed. Volumes XI and XII in progress.

[ANY.—W. A. Setchell, Editor. Price per volume $3.50. Volumes I (pp. 418), II (pp. 354), III (pp. 400), and IV (pp. 397) completed. Volumes V and VI in progress.

SSICAL PHILOLOGY.—Edward B. Clapp, William A. Merrill, Herbert C. Nutting, Editors. Price per volume $2.50. Volume I (pp. 270) completed. Volumes II and III in progress.

NOMICS.—Adolph C. Miller, Editor. Volumes I (632 pages, price $5.00), II (461 pages, price $4.00), and III (451 pages, price $3.50) completed. Volume IV in progress.

JCATION.—Alexis F. Lange, Editor. Volumes I (424 pages, price $3.50), III (447 pages, price $3.50), and IV (258 pages, price $2.50) completed. Volumes II and V in progress.

TORY.—Henry Morse Stephens and Herbert E. Bolton, Editors. Volumes I (pp. 406), $3.00, II (pp. 278) $2.00, III (pp. 510) $3.50, carriage extra.

THEMATICS.—Mellen W. Haskell, Editor. Price per volume $3.50. Volume I in progress.

DERN PHILOLOGY.—Charles M. Gayley, Hugo K. Schilling, Rudolph Schevill, Editors. Price per volume $2.50. Volume I (pp. 400), II (pp. 373) completed. Volumes III and IV in progress.

MOIRS OF THE UNIVERSITY OF CALIFORNIA (Quarto).

Vol. 1. 1. Triassic Ichthyosauria, with Special Reference to the American Forms, by John C. Merriam. Pp. 1-196, plates 1-18. September, 1908 .... $3.00
2. The Fauna of Rancho La Brea. Part I. Occurrence, by John C. Merriam. Pp. 197-213, plates 19-23. November, 1911 .... .30
3. The Fauna of Rancho La Brea. Part II. Canidae, by John C. Merriam. Pp. 215-272, plates 24-28, 35 text figures. October, 1912 .... .80

Vol. 2. The Silva of California, by Willis Linn Jepson. 480 pages, with 85 plates and 3 maps. December, 1910. Sewed, in paper covers, $5; bound in cloth, $7.50. (By express, $8.40.)

Vol. 3. Business Cycles, by Wesley Clair Mitchell. xviii + 610 pages, with 77 charts. September, 1913. Sewed, in paper covers, $5; bound in buckram, $7.50. (Expressage, 50 cents extra.)

Vol. 4. 1. The Battle of the Seven Arts, a French Poem by Henri d'Andeli, Trouvère of the Thirteenth Century. Edited and Translated, with Introduction and Notes by Louis J. Paetow. Pp. 1-63, plates 1-10. July, 1914 .... .75

ther series in Agricultural Sciences, Botany, Engineering, Entomology, Geography, ogy, Physiology, Pathology, and Zoology.

VERSITY OF CALIFORNIA CHRONICLE.—An official record of University life, issued quarterly, edited by a committee of the faculty. Price $1 per year. Current volume No. XVIII.

IINISTRATIVE BULLETINS OF THE UNIVERSITY OF CALIFORNIA.—Edited by the Recorder of the Faculties. Includes the Register, the President's Report, the Secretary's Report, and other official announcements.

ddress all orders, or requests for information concerning the above publications to University of California Press, Berkeley, California.